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Associate Editor

EUNICE THOMAS MINER, Executive Secretary

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 3 DECEMBER, 1940 No. 2

SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 4, 1940

DOCTOR MAX DEMOREST, Department of Geological Sciences, Yale University; *The Rock Called Ice.* (This lecture was illustrated by lantern slides.)

The petrologist has long recognized that ice is a rock, but having made that recognition he has done little to make an acquaintance. Among geologists, therefore, the task of studying this rock has fallen to the geomorphologist; and this is to be expected, for most of the natural occurrences of ice are at the earth's surface where they are geologically important as factors in the erosional and depositional history of many land forms. He studies the igneous rock called ice when he finds it in winter on the surfaces of our ponds and lakes. There he is interested in the pressure of expanding ice and the shoreline ramparts it builds. He studies the sedimentary rock called ice when he finds it in the perennial snows of high altitudes and high latitudes. There he is interested in nivation effects and the peculiar erosional processes that are associated with such snows. Of even more interest are the larger areas of snow—the névé fields which are the feeding grounds of glaciers; while in the glaciers themselves he finds the metamorphic rock called ice, for the sedimentary snow and firn

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of the névé is very truly metamorphosed in the process of flowage which causes a continuous movement of ice from the head toward the snout of a glacier.

In studying this latter subject—the flowage and metamorphism of ice—the geomorphologist finds himself in the fields of structural petrology and applied physics; and he finds himself in need of techniques that he has hitherto neglected. The petrologist in working on the metamorphism of other rocks has made constant use of the petrographic microscope and, within the last two decades, has developed the technique of petrofabric analysis wherein a universal stage is used to make statistical studies of crystallographic and dimensional orientations of mineral grains in rocks. An attempt is made to relate these orientations to the forces thought to be responsible for the deformation and metamorphism of the rock. More recently, the petrologists have conducted experimental studies with a view toward the laboratory production of rock flowage under controlled conditions of temperature, static pressure, and deformational stress.

Thanks to Yale University, the National Research Council, the Geological Society of America, and the Society of Sigma Xi, I have been privileged to pioneer in the application of these techniques to the study of ice deformation. However, the fact that ice is a rock of rather peculiar properties has demanded that the techniques be modified in some respects. The most obvious peculiarity of this rock is its low melting point, a fact that makes it necessary to conduct all studies and experiments at a temperature below 32° F. While this makes no fundamental difference in the methods used, it does work a hardship on the fingers of the student and makes long woollen underwear a scientific necessity. Another peculiarity of ice is the large grain size which characterizes it in many of its forms. Igneous ice on lakes may contain individual grains of several inches across, and the metamorphic ice of a glacier is frequently made up of grains measuring up to six inches. For the study of such large grained material it is obvious that the ordinary microscope and universal stage of the petrographer is much too small, so it has been necessary to construct special apparatus. The universal stage now in use has a diameter of fourteen inches and will take thin sections measuring half that distance across. Seven inch discs of polaroid are used as polarizer and analyser.

Fortunately, the birefringence of ice is so low (0.004) that sections need not be ground as thin as for other rocks. Twenty to thirty thousandths of an inch is usually sufficient, and, since ice is a soft material, sections can be ground rapidly. The method of making sections is essentially the same as that used by petrographers, but a common hand saw takes the place of the expensive rock saw. Coarse grinding is done with a rotary wire brush attached to an electric motor and the finishing is done on emery cloth laid flat on a smooth board. Instead of balsam cement, a little water is applied around the edges of the specimen with an eye dropper. No cover glass is used since the sections, being relatively thick, are not fragile, and it is not practical to preserve them for any great length of time. Yet, in the subfreezing laboratory, they can be conveniently stored for many months in kerosene where evaporation is prohibited.

Thus far, the petrologic studies of ice which I have conducted have been of two sorts:—first, laboratory experiments on the deformation of ice; and second, field studies of deformed ice in glaciers. In the experimental work, small cylinders of ice are cut from large single crystals found in clear lake ice. Each cylinder is mounted in a chamber where controlled high static pressures may be applied and the temperature regulated to any desired value. Then deformational pressures are applied which force the ice to flow by slowly squashing it. By separately varying the pressure, temperature, and deformational stress over a large series of tests the effect of each of these factors on the flow of ice may be evaluated. Thin section studies of cylinders removed from the apparatus after different degrees of deformation reveal the nature of the crystallographic changes which develop as flow progresses. To fully study the subject by this method, hundreds of specimens should be tested—a task requiring several years. As yet, only preliminary studies are under way, so no definitive results have been obtained.

The field work has been done at Mt. Rainier where numerous glaciers supply an abundance of material, and where the National Park Company kindly put a cold storage room at our disposal. Samples of ice were collected from glaciers at localities where visible structures seemed to show the probable distribution of deforming stresses. Back in the cold room, these samples were cut in thin section and petrographic studies made. As with the laboratory work, there is more to do in the future than has yet been done. Nevertheless, interesting results have been obtained. Most important of these results is a demonstration that ice flowage proceeds by a process of recrystallization. In this process, the small, approximately equidimensional grains of the névé are transformed into large, irregular, interlocking grains that fit together like pieces of a three dimensional jig-saw puzzle, and the flowage of ice results from a continuous adjustment of grain forms to accommodate the stresses. While recrystallization has previously been suggested as the principal process of glacier flow, the exact nature and method of that recrystallization have not been understood hitherto; nor have the present studies brought certainty to our understanding, though we have formed tentative conclusions which seem to offer a satisfactory explanation.

While these conclusions were orally discussed in the lecture, a published discussion of them now would be premature. This is especially so, since experimental tests are in preparation which will allow us to see and photograph thin sections of ice under high confining and shear stresses. The ice will be forced to flow, while, by means of polarized light, we watch and take moving pictures of the progression of crystallographic changes. If we should see what I think we shall see, we shall know that our tentative conclusions are right. If we see something else, we shall know either that our conclusions are wrong, or that the apparatus does not produce flow conditions similar to those in a glacier, and that therefore the experimental flow is of a different sort. Perhaps we shall find that both the flow conditions are different and the conclusions wrong. In any case, published discussion should await these experimental tests.

SECTION OF BIOLOGY

November 12, 1940

Doctor Dugald Brown, New York University and Bellevue Hospital Medical College: *Biological Reactions to High Pressure.* (This lecture was illustrated by lantern slides.) No abstract has been received by the Editor.

SECTION OF PSYCHOLOGY

November 18, 1940

Professor Robert S. Woodworth, Columbia University: *Recent Results on Heredity and Environment.*

The pioneer student of twins, as to the light their resemblances and differences throw on the effects of heredity and environment, was Sir Francis Galton. Of recent years, with the development of reliable methods of distinguishing monozygotic or "identical" twins from dizygotic or "fraternal" twins, this line of investigation has received new impetus. Identicals, formed by the splitting of a single individual very early in embryonic life, have the same genes, the same heredity, while fraternals differ in heredity by a variable amount, just as other siblings do. The "twin method," developed in Germany and much used there during the past two decades, proceeds as follows: Find the average difference between identicals in a certain trait. This difference is due to environment, since identicals are alike in heredity. Find the average difference between fraternals also. This difference is due to both heredity and environment, and is practically always greater than the difference between identicals. Now, subtract the difference between identicals (due to environment alone) from the difference between fraternals (due to both heredity and environment), and the remainder is an index of the importance of heredity in making individuals different in the given trait. An assumption underlying the use of this formula is that the environment is as much alike for a pair of fraternals as for a pair of identicals. This assumption is probably justified in respect to the bodily traits, where the twin method has mostly been employed. But when the German psychologists began to use the method for the study of intellectual and character traits, they soon became aware that the assumption was rather dubious. Inquiry in this country (by P. T. Wilson) has shown that identicals are together much more than fraternals, have the same chums, play the same games, and in other respects have a more common environment than fraternals. So this twin method cannot be used with much precision for psychological investigations. The question remains, and will be alluded to later, as to *why* the environment should differ more for fraternals than for identicals. Meanwhile, the German psychologists, by a more or less cautious use of the method, have made it probable that character differences depend on heredity as well as on environment.

For a time, twin research was very active in the USSR, and the Russian psychologists were using a form of the "co-twin control" method, somewhat like that of Gesell. Take a pair of identicals, and subject one twin to a certain environmental factor, i. e., one kind of training, the other twin to an alternative factor, and see which twin comes along better. Do this, of course, with a considerable number of identical pairs. Some good results were being obtained by this method, when a halt was called by the political authorities. The method seems quite suitable for testing out the effect of environmental factors; and it must be admitted that we now know very little regarding the influence of different environments upon intelligence and personality.

In this country, an outstanding twin study has sought to find twins who have been reared apart since infancy. The method here is another form of co-twin control. Newman, Freeman and Holzinger (1937) were able to find 19 pairs of identicals reared apart, usually in not very diverse environments, but, in a few cases, receiving very different educational advantages. One young woman who had been through college showed an IQ of 116, while her twin who had had only the first two grades in school got 92. This difference of 24 points was the largest found, but there were three other cases in which the twins had received very unequal educational advantages and in which there were corresponding IQ differences measuring 19, 17, and 12 points. In the remaining cases, the twins had been reared under conditions differing about as they differ in the homes of an ordinary community, and we might expect the twins of a pair to differ about as much as two children taken at random from such a community. Actually, they differed on the average by about 5.5 points, almost exactly as much as found by these authors in a sample of 50 pairs of identical twins brought up together. In fact this is about the same difference as is found on the average between test and prompt retest of the same individual. Taking pairs of unrelated children from the community, we should find them to differ on the average by much more—about 15–18 points. Though the sample of identicals reared apart is still pretty small, two tentative conclusions seem to be justified. First, a large difference in educational environment can produce quite an appreciable difference in tested intelligence. Second, the differences in intelligence found in an ordinary community are not accounted for to any great extent by differences in home and school environment; for, if they were, children having identical heredity, but brought up in different homes, should differ as much as children paired at random from the community.

Broadening the statement somewhat, we are probably justified in saying that differences in intelligence, as we find them in members of the same family or ordinary community, are due in large measure to differences in native capacity, but that, nevertheless, raising the level of the environment (if we knew how to do it) would improve the intelligence of the population in general. These two statements are not contradictory in the least. When the environment is fairly uniform, the differences present in the population must be due mostly to heredity, as Shuttleworth has said (1935). The same author has made another point which deserves emphasis. Heredity and environment interact. Parents giving their children the better heredity will on the whole give them the better environment. Within the same family, we may add, children differing by nature are sure to be treated differently, and they are sure to select differently from among the varied opportunities offered by the environment. Here, we see the reason, probably, why the effective environment differs more for fraternal than for identical twins. Unlike heredity is the primary factor, but unlike heredity operates largely by producing differences in the effective environment.

Another important twin study is that of Rosanoff, Handy & Plesset (1937). Starting from a large sample of mentally deficient individuals who were known to have twins, brothers or sisters, they located those twins and determined their mental level and condition. What per cent of them were normal? In the case of identicals, only 9 per cent; with same-sex fraternals, 39 per cent; with opposite-sex fraternals, 53 per cent; as against 84 per cent of the siblings of mentally deficient individuals. Analysis of these per cents and of other evidence leads the authors to place strong emphasis on prenatal and natal conditions as factors in the causation of mental deficiency. Twins—one or both of a pair—are especially exposed to prenatal malnutrition and birth injury. When one twin of a pair of identicals is normal and the other feebleminded, the normal one proves that the other has the genes for normality. The feebleminded one, for some reason, has not come up to his hereditary potentiality, and the reason is often found in birth injury or prenatal maldevelopment. Rosanoff and his associates believe that minor degrees of brain injury often occur without being noticed at birth. In this way they would explain the occasional cases of identical twins who differ considerably in ability, in spite of having been reared together. They found several such cases among superior children even. One twin in a certain pair of identicals had an IQ of 150, proving that his twin had the genes to reach the same level; but the other's IQ was only 118, which, even though rather high, showed what these authors call "relative mental deficiency," i. e., failure to develop his full hereditary potentiality. Those who use twins for the study of educational and social environment need to be on their guard against disturbance by such physical factors as these authors have stressed.

Besides the twin studies, another main line of investigation consists in removing children from one environment to another, and observing and measuring, so far as possible, the resulting changes in their ability and personality. Of many studies of this type—studies of foster children and of the effect of superior schools and nursery schools—, those recently reported from the

State University of Iowa have attracted most attention, because of the strong claims made for environmental effects. The long-established and excellent nursery school conducted by the University reports an average gain of at least 10 points in IQ resulting from attendance, and the gain in some individual children has been much greater than this. As far as the average is concerned, this result has not been confirmed by several other excellent nursery schools. The Iowa school may be better, or the home environments may be better in the other places, for the child does not come to the nursery school from a vacuum. We may hope that the Iowa school is better and that it well be possible to analyze out the points of superiority, and so discover what sort of early environment is best for intellectual and personality development. As to the individual children who made such large gains, what we need is careful case studies rather than simply a notation of the child's IQ at different ages. If emphasis is to be placed on the individual case, we should know the child's parentage and home environment, and the nature of his improvement in school.

An orphanage, so it would seem, makes a favorable situation for a nursery school experiment, because the usual orphanage environment is not intellectually stimulating to the children. The Iowa psychologists set up a nursery school in such an orphanage, but encountered one serious difficulty. The more promising children were constantly being placed for adoption, so that it was impossible to apply the nursery school environment for a two or three year period to any large group of children. The results, accordingly, were not very clear, though they did seem to show some gain for the nursery school children.

Foster children, also, are being studied by the Iowa group, and they find, as others have found in other States, that foster children turn out encouragingly well. Placement officers, it should be understood, are under moral and legal obligation not to place for adoption any children who are especially poor risks because of parentage, behavior, mental retardation, or medical condition. This restriction makes it difficult to conduct a conclusive experiment. But the evidence points to some gain in IQ when children are transferred from poor homes or from the orphanage to good foster homes. At last testing, the children in the Iowa sample were still under five years old on the average, and, as they are being followed up, it is best to delay conclusions. This caution applies to the particular sub-sample of 16 children of feebleminded mothers (average IQ of mothers, about 70; average schooling, 7.6 grades; average schooling of fathers, 9 grades). These children were placed for adoption, in spite of the restrictions above mentioned. After adoption, none of these children turned out to be feebleminded, their average IQ being 116 when they were two years old, and 108 when they were four. This result does not warrant the general statement that children of feebleminded mothers are perfectly safe for adoption. The sample was small and obviously selected (by the placement officials) and the nature of the mother's deficiency is not stated. If a mother is defective because of birth injury, there is no reason to expect her child to be defective. With our increasing knowledge of varieties of feeblemindedness, it is necessary to draw distinctions that were not considered formerly.

There are several other studies of the Iowa psychologists which should be included in a complete survey. In general, this group is to be commended for formulating bold and definite hypotheses, but it is only fair to say that their claims still have the status of hypotheses. The time is not ripe for reaching agreement on the nature-nurture problem, but it is ripe, distinctly, for incisive and well-controlled research.

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ELECTED NOVEMBER 4, 1940

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SECTION OF BIOLOGY[1]

JANUARY 13, 1941

PROFESSOR HORACE ELMER WOOD, 2ND, University of Newark; *Trends in Rhinoceros Evolution.* (This lecture was illustrated by lantern slides.)

It is particularly appropriate that I submit this progress report to the New York Academy of Sciences, since a grant from the Esther Hermann Fund of the Academy, sixteen years ago, was the first outside support for this investigation. More recently, it has been assisted by a grant from the Penrose Fund of the American Philosophical Society.

There is a fable that a publisher once envisioned a definitive book on rhinoceroses. He therefore employed an Englishman, a Frenchman, a German, a Russian and a Pole, each to prepare a suitable manuscript, stipulating the right to publish the best manuscript or to combine excerpts from the several authors. The Englishman disappeared into the wilds of Africa and the Oriental Region, returned with photographs of himself leaning on his gun beside slain rhinoceroses and submitted a manuscript on "How to Hunt Rhinoceroses." The Frenchman, after some

[1] No meeting was held in January by the Section of Geology.

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afternoons spent in the Jardin des Plantes, turned in a short romance, "Les amours des rhinocéros." The German read and partially digested everything that had been published about rhinoceroses and turned in 6,000 manuscript pages under the title, "Ein kurtzes Handbuch der Rhinozeroskunde." The Russian retired to his study with adequate supplies of vodka and tea, and after a few days turned out a philosophical discussion entitled, "The Rhinoceros, does he exist?" And the Pole, after a similar period of isolation, fortified with tea and malinowka, submitted a controversial pamphlet, "Rhinoceroses and the Polish Question." In addition to these aspects, I must omit recognition of the long service of rhinoceros blood, pulverized teeth, horn and other odd parts in the Chinese pharmacopoeia, or the protection of western civilization by cups carved from rhinoceros horn as a sure preventive of, or antidote to poisoning—as long as no poison was used. I shall limit myself to well authenticated data and conclusions, however tempting some of the less established inferences may be, and shall make no attempt to assign personal credit for the various discoveries and generalizations.

The rhinoceroses are generally ranked as a superfamily of the Order Perissodactyla, those hoofed mammals with the mid-line of the foot passing through the third toe. The other living representatives of the order are the tapirs and horses, and the major extinct groups are the titanotheres, chalicotheres and lophiodonts. The living perissodactyls have reached very different stages of evolution. The tapirs are living fossils in an Oligocene stage of evolution, and only the horses may be considered as legitimate participants in the current scene. Of the five living kinds of rhinoceroses, the Sumatran rhinoceros represents a Miocene stage of evolution; the Javan, Indian and black rhinoceroses, various Pliocene stages; and the white rhinoceros would not have been an anachronism in the Pleistocene. Rhinoceroses first appeared in the Eocene, and they have occupied every continent except Australia and South America, and are as common in most of the North American Tertiary as in the old world. The chief evolutionary changes have been in teeth, horns, size and bodily proportions, whereas the feet and the general character of the skeleton have remained rather stable.

In studying the evolutionary history of the rhinoceroses, we are not merely investigating one more instance of evolution at work, but one which supplies important supplementary information and corrects some current misconceptions. Due to our relatively complete knowledge, the phylogeny of the horse has inevitably come to be the standard textbook example of evolution. Nevertheless, this use has encouraged misinterpretation of the evolutionary process. The phylogeny of the horse is constantly cited to illustrate orthogenesis; that is, using the tree metaphor, the family tree is said to resemble a pine tree with one main stem and insignificant side branches. This has then led further still to the assertion that evolution "usually," or even, "inevitably" proceeds in straight lines, which are thereupon considered as in some Presbyterian fashion predestined or orthogenetic. Again, some particularly spectacular descendant is supposed to be somehow more definitely "predetermined" by the common ancestor than other less interesting issue, like a commercial family tree which shows direct lines of descent from William the Conqueror and Queen Elizabeth culminating in the purchaser. As a matter of fact, the horse family tree is not nearly so simple as it is usually stated to be, as shown even by Matthew's simplified phylogeny, and Stirton's recent phylogeny is still more complex. Although these family trees are anything but monophyletic, it is nevertheless a fact that they are truly simple in comparison with most others. Phylogenetic trees of the giraffes by Colbert, of the North American Canidae by Matthew, and of the higher primates by Gregory show considerably more branching (the diversified adaptation of Darwin or adaptive radiation of Osborn). My rhinoceros phylogeny of several years ago is still more complex, and would be further complicated by recent discoveries. An orthogenetic interpretation of such involved ramification recalls Ouida's messenger who "jumped on his horse and galloped off in all directions." Also, the horses, with their unusually simple family tree, and the rhinoceroses, with their extraordinarily complex one, belong to the same order and should be affected by the same fundamental biological principles. If a special entelechy is required to explain the comparatively simple horse phylogeny, we should need another polyphyletic entelechy to account for the complex rhinoceros family tree, and we should, I suppose, also expect several run-of-the-mill daemons to account for more typical phylogenies of intermediate type. Anyone who believes that such logomachy explains anything is, of course, welcome to any satisfaction he derives therefrom. It seems, however, as if phylogenies, like many other summations of a large number of variables, fall into a normal curve of distribution with few either really simple or remarkably complex, but most of them of about average complexity.

Let us trace some of the main trends in rhinoceros evolution. The primitive stem group, the Hyrachyidae, springing from a common stock close to the ancestry of the horse, represents an Eocene adaptive radiation of the ancestral or, more probably, near-ancestral stem rhinoceroses. They ranged from the size of a wolfhound to that of a mustang and were moderately cursorial. The molars were only subrhinocerotic.

The hyracodonts are an Eocene-Oligocene line with fully rhinocerotic cheek teeth, but otherwise not enormously modified from the hyrachyid condition. They are slightly more specialized for cursorial life and roughly parallel the contemporary horses. It is a plausible assumption that the hyracodonts succumbed to competition with their rivals.

The amynodonts have generally been recognized as a well-defined taxonomic group, now usually given family rank. They become, progressively, more heavily built, more specialized for semiaquatic life in hippopotamus fashion, and more achondroplastic. Although it seems surprising that achondroplasia should ever evolve, achrondroplastic strains of fishes, cattle, cave-bears and bull-dogs have actually maintained themselves for varying periods. The teeth show progressive atrophy of the incisors and premolars and hypertrophy of the canines and molars, with what simulates a plastic deformation of the molars by bucco-lingual

		California		Great Plains and Rocky Mountain Basins	
Oligocene	Whitneyan	No amynodonts known			
	Orellan			Orella	Upper *Metamynodon* sp., cf. *planifrons* but more advanced Lower *Metamynodon planifrons*
	Chadronian			Chadron	*Metamynodon chadronensis*
Eocene	Duchesnean	Sespe	*Amynodontopsis bodei*	Duchesne River	*Amynodon*, n. sp., intermediate between *A. intermedius* and *M. chadronensis*
	Uintan		*Amynodon* sp.	Uinta	C *Amynodon intermedius* intergradation B *Amynodon advenus* and *A. reedi* A *Amynodon advenus*
	Uintan or Bridgerian or transitional	Poway	*Amynodon reedi*		
	Bridgerian Wasatchian	No amynodonts known			

squeezing, with some antero-posterior shear, and hypsodonty of the external margins of the molars, the whole being a remarkable parallel to the extinct South American astrapotheres.

This table shows the stratigraphic distribution of all known American amynodonts. No intergradation has been observed in this sequence, except between *A. advenus* and *A. intermedius*, where there is either stratigraphic or morphologic intergradation, according to one's concept of what a species is in paleontology. That is, if species are defined strictly by their morphology, one stray *A. intermedius* is known from Uinta B and several members of *A. advenus* have been collected from a quarry in Uinta C. On the other hand, if species are defined partly in terms of their geologic levels and (purely theoretical) breeding opportunities, then the two species in question can be separated stratigraphically but overlap somewhat in structure. It follows logically that if the question of what is a "real" species has become so nearly meaningless in paleontology, the question as to what is a "real genus" is several degrees less tangible. The only useful criterion to follow is the greatest convenience of the greatest number. There is virtually a unitary succession of progressively more specialized types at each ascending geologic level. Except for the occurrence of *Amynodon reedi* (the most primitive species) in Uinta B as well as in the probably earlier Poway conglomerate of California, and the aberrant Californian genus, *Amynodontopsis*, this series is a made-to-order example to prove orthogenetic evolution. I know of no similar authentic case so completely devoid of side lines, in the whole field of paleontology, in which an internal perfecting principle seems so clearly at work. Although I see no compelling evidence in favor of orthogenesis, I present this striking case of predetermined evolution working through the stages of racial youth, maturity and old age to the predestined doom of racial senescence and extinction. However, any mystical interpretation of the monophyletic American series must also necessarily take account of the entirely different evolutionary pattern found in the Mongolian amynodonts. Preliminary study of the extensive American Museum collection shows a very different state of affairs—a complex, polyphyletic type of evolution diverging into numerous lines, which frequently parallel each other and the American amynodonts, forming five main autochthonous lines (if "main" is an appropriate term) during the upper Eocene and the Oligocene, in addition to an undescribed species of *Metamynodon*, presumably an immigrant from North America. The only forms yet described are the relatively primitive *Amynodon mongoliensis* and the so-called "*Cadurcotherium*" *ardynense*, a rather specialized form.

I prefer a strictly neo-Darwinian interpretation along the lines under exploration by Fisher and Wright. That is, a finite but extremely large number of mutations are potentially possible for any particular stock at any one time. Of such chemically possible mutations as may actually happen to occur, the vast majority prove to be anatomically or physiologically maladjusted and, hence, are unable to become widely established in the population. The residue, which, while proportionately a small number, would normally be a large absolute number, represent the possible next steps in evolution. Whether a given population follows one trend or several, or many divergent trends, would then largely depend on whether one chemically possible and genetically not too improbable line of evolution is sufficiently superior adaptively to its competitors to eliminate them promptly (for example, before they get far enough for their fossil remains to be likely to be found and recognized by paleontologists), or whether several or many alternative evolutionary lines are about equally efficient in maintaining themselves, especially by occupying non-competitive ecologic niches.

The gigantic Asiatic baluchitheres became the largest known land mammals, standing several feet higher than the largest elephant. The third metapodials are more enlarged, relative to the second and fourth, than in any other rhinoceros. *Forstercooperia* was recently described from the upper Eocene Irden Manha formation of Mongolia as a possible ancestor.

Eocene true rhinoceroses have been reported from both Europe and North America, but, so far, either the classification, age, or both, have been challenged. One general common stock of hornless cursorial true rhinoceroses is found in the early Oligocene of both Europe and North America, represented by numerous forms, sometimes closely related, which are usually called genera. *Trigonias* is the best known of the more primitive forms. The manus is tetradactyl, and up to 42 teeth may be present in the jaws. Unsupported dogmatic statements have been made that this geographic distribution proves that the original true rhinoceroses were old world forms and spread thence to the new world, or vice versa, or that they must have lived in Asia and simultaneously invaded Europe and North America. Any of these statements go beyond the data, but Europe and North America were occupied by one common stock, wherever it may have originated. From then on, the most striking process was evolution in place.

The following series are probably valid or nearly so:

North America

Lower Miocene (various formations and regions)			—*Diceratherium armatum* (horns in male)
			moderate change
Oligocene	Upper—Whitney member of Brule		—*Subhyracodon tridactylus* (horn nubbins sometimes present, probably in male)
			intergradation
	Middle—Orella member of Brule	upper	—*Subhyracodon* "*metalophus*"
			intergradation
		lower	—*Subhyracodon occidentalis*
			intergradation
	Lower—Chadron formation	upper	—*Subhyracodon copei*
			no intergradation
		middle	—*Trigonias* (probably not the direct ancestor)
		lower	—?

Once more, the intergradation which occurs here can be described as anatomical or stratigraphic. That is, if all the individuals at one level are called a "species," successive species overlap somewhat structurally; or, if "species" are defined structurally, they overlap somewhat stratigraphically. *Diceratherium* left no known descendants. However, the later American rhinoceroses, *Aphelops* and *Peraceras* (but not *Teleoceras*) may have descended from *Subhyracodon*.

EUROPE AND ADJOINING TERRITORY

		European dicerorhines	Aceratheres
Pleistocene	Magdalenian Moustierian	*Coelodonta antiquitatis*	
	Acheulean	"*Dihoplus*" *mercki*	
		intergradation reported	
	Chellean	"*Dihoplus*" *etruscus*	
	Sicilian	"*Dihoplus*" *etruscus*	
Pliocene	Astian	"*Dihoplus*" *leptorhinus*	
	Plaisancian	"*Dihoplus*" *leptorhinus*	
	Pontian	"*Dihoplus*" *schleiermacheri*	*Aceratherium incisivum*
Miocene	Vindobonian	"*Dihoplus*" *hispanicus* "*Dihoplus*" *simorrensis*	*Aceratherium tetradactylum*
	Burdigalian	"*Dihoplus*" *sansaniensis* "*Dicerorhinus*" *tagicus*	*Accratherium depereti*
	Aquitanian	"*Dicerorhinus*" *tagicus*	*Aceratherium lemanense*
Oligocene	Upper Stampian	"*Dicerorhinus*" *tagicus tolosanus*	*Aceratherium lemanense*
	Middle Stampian	*Protaceratherium albigense*	

The nomenclature of the European dicerorhines is in chaotic condition and the particular names here recognized could be criticised very readily. However, the succession itself seems to be real. The acerathere series is slightly modified from the long-cited sequence.

In a number of cases (e. g., *Amynodon* in the upper Eocene of Asia and North America), intercontinental migration of some sort must have taken place. There are also a large number of dubious cases. But in only one case so far published is it possible to demonstrate the direction of migration. The genus *Brachypotherium* is represented by successively more specialized species from the earliest Miocene into the Pliocene of Eurasia and north Africa. A direct derivative, *Teleoceras*, appears in the upper Miocene of North America and extends through the lower and middle Pliocene, represented by successively more specialized forms. So similar is a primitive *Teleoceras* to a moderately specialized *Brachypotherium* that assignment between the two genera is based simply on locality, so that essentially indistinguishable specimens from Japan and Nebraska have been assigned without objection to *Brachypotherium* and to *Teleoceras* (*Mesoceras*).

Students of migration have frequently commented upon two striking and unexplained failures to grasp available opportunities.

When, in the Pliocene, the Panama land-bridge was reestablished, rhinoceroses did not accompany horses, elephants and tapirs across it into South America, although they survived long enough to coexist with northwardly migrating ground sloths in at least four middle Pliocene (Hemphillian) local faunas in the United States. No one has ever understood why the woolly rhinoceros did not accompany the boreal mammoth across the Siberia-Alaska bridge into North America in the Pleistocene. No explanation can be assigned for the latter failure, since the bridge should have been a "corridor" for both, and only the elastic formula, climatic barrier, for the first. The failure of the Pleistocene elasmotheres to reach North America is less surprising, since they seem to have been restricted to the middle latitudes and may not have ranged far enough north to have been able to use the bridge.

The rhinoceroses show interesting examples of parallel and convergent evolution. Compare the familiar evolution of horse cheek teeth with the progressive development of comparable plications and hypsodonty in the cheek teeth of the extinct elasmotheres of Eurasia. The end stage almost exactly mimics the horses, or even out-horses them, in the length of crown and the lateness of the development of roots (thus closing the pulp cavity); each tooth having become a complicated, high-crowned, grinding mill with alternating areas of enamel, dentine and cement. Nevertheless, this end stage is still a gloss on the fundamental rhinoceros pattern, representing a Greek letter *pi* in the upper molars and two sharply asymmetrical crescents in the lower molars, and never acquired a true horse pattern. Although this convergence extends only to the cheek teeth, it is quite clear that one or both are guilty of flagrant plagiarism. The end stage (*Elasmotherium*) was the largest of all rhinoceroses except the baluchitheres and bore a single enormous frontal horn. The elasmotheres were, for most of the time since their discovery, a "great unknown," and have been assigned relationship to edentates, carnivores and aquatic mammals.

Parallel evolution also occurs among the rhinoceroses. One of the most striking cases is the resemblance between the extinct woolly rhinoceros of the Eurasiatic glacial period and the living white African rhinoceros, in size, body shape, horns and hypsodonty. Compare, also, the Dordogne cave engraving with rock carvings by African bushmen. So close a resemblance has usually been interpreted as due to direct descent, the white rhinoceros being considered a southern survivor of an extinct northern relative, just as the African hippopotamus is considered a little changed representative of the Pleistocene hippos of Europe. However, the evidence now justifies the conclusion that the woolly rhinoceros was, as previously indicated, the terminal member of a northern line of Sumatran rhinoceroses represented in each successive level of the later Cenozoic of Europe, while the white rhinoceros appears to be a more specialized, grazing relative of the existing African black rhinoceros, with an autochthonous African line of evolution, which, although largely unknown, must have been fairly distinct from the line leading to the woolly rhinoceros. This particular case of parallelism is almost as well documented anatomically as if both forms were extant, since the woolly rhinoceros is known not only from abundant ordinary skeletal remains, but also from five "cold-storage" carcasses from Siberia and two eviscerated carcasses from Galicia, "pickled" in brine and hydrocarbons.

The scientific validity of family trees has often been challenged. Inevitably, they suffer from oversimplification. Conversely, it is sometimes charged that they embody artificial complications, such as the theoretical descent of imaginary groups based on taxonomic recognition of the extremes of individual variation found at successive levels, with imaginary genetic relationship of these extremes. With all allowance for the only too frequent validity of both criticisms, there is, nevertheless, a considerable field of usefulness for carefully drawn family trees, and I know of no alternative which permits the expression of data in more significant and usable form. As a check, I reexamined the older rhinoceros family trees. The first, Cope's tree, dating from 1879, is essentially a comparative anatomy diagram without detail. The first detailed phylogenies, Gaudry's of 1888 and Pavlow's of 1892, are more readily compared with modern views. Although there are great differences, particularly in the enormous expansion of the American record and in the completely new record from eastern Asia, whole sections from the European parts of Gaudry's and Pavlow's trees survive in my phylogeny with little change. A later, more balanced treatment of new and old world forms will be found in Osborn's 1905 phylogeny, which summarizes the results of his concentrated work on rhinoceroses, and in his unpublished 1926 phylogeny, which is partly a compromise with my views of that time. Matthew, in 1931 and 1932, expressed considerable disagreement with Osborn and with Wood. Nevertheless, there is a fair measure of essential agreement, aside from temperamental differences as to what to call things. My last effort at a phylogenetic tree has not been published and already seems out of date and amateurish. Nevertheless, most of it would appear without essential change in any revised version. It, therefore, seems that, when one related species follows another in ascending stratigraphic sequence in a natural geographic region, this may approximately represent the real succession, and further discoveries are more likely to confirm than to overturn this conclusion. On the other hand, a series based on scattering forms with no logical stratigraphic or geographic relation to each other should be regarded as only a morphologic series comparable to those studied by a student in comparative anatomy and, hence, as likely to suffer extreme reinterpretation as additional data accumulate.

Many problems are still to be solved. Besides working out, in greater detail, questions for which the probable solution is already indicated—"carrying it out to another decimal point"—the following are a few obvious ones:

1. The exact ancestry of the rhinoceroses in general and their exact relationship to other primitive perissodactyls.
2. The exact ancestry of the amynodonts, of the baluchitheres, and of the true rhinoceroses (Rhinocerotidae).

3. The ancestry of certain late American forms (*Aphelops* and *Peraceras*). Whether autochthonous evolution, immigration, or both are hereafter demonstrated, the answer will be equally interesting.
4. The ancestry of the living African rhinoceroses. Essentially indistinguishable forms have been reported in the African Pleistocene. The only earlier hint is *Diceros* (?) *pachygnathus* from the Pontian of Pikermi. A still earlier ancestry from American forms or their old world relatives is conceivable, but hypothetical to an extreme.

The future of the rhinoceroses seems dark indeed. In the past, they occupied every continent except Australia and South America, but their range has been shrinking rapidly for the last few million years. The last American rhinoceroses, *Aphelops* and *Teleoceras*, date from the middle Pliocene. In Europe, rhinoceroses survived to the end of the Pleistocene and were represented by Crô-Magnon artists. Although the reactions of early man to rhinoceroses are thus preserved, we have no data on the converse reaction. Judging from the behavior of their living relatives, it was probably a blind charge or an equally panicky flight. Rhinoceroses survived in south China down to the historical period, perhaps to 200 A. D. or even, by one account, to a thousand years later. Linnaeus, with less than his usual acumen, classified the Indian rhinoceros as a rodent. But, two centuries earlier, the Mogul conqueror of India, Babur (or his literary "ghost"), concluded that the rhinoceros resembled the horse more than any other animal, especially in stomach and foot. At that time, rhinoceroses were abundant across northern India, down the Malay Peninsula and in Sumatra, Java and Borneo. At about the same time, the first African explorers found rhinoceroses abundant south of the deserts and west to Nigeria. Since then, rhinoceroses have not bowed to the increasingly tyrannical human domination of the land surface of the earth, and three centuries of close settlement and sportsmen have restricted the white rhinoceros to the game reserve in Zululand and to the Lado-Uganda region of equatorial Africa, with a hundred or so individuals in each area. The black rhinoceros, still reasonably abundant in parts of south, central and east Africa, has been considerably restricted in range. Due to their financial value for "medicinal" purposes, all three Asiatic species are on the verge of extinction, according to the game wardens. Perhaps less than a dozen individual Javan rhinoceroses are still alive, and perhaps a hundred Indian rhinoceroses, while living Sumatran rhinoceroses would more probably be numbered in hundreds than in thousands. The situation is made more hopeless by the fact that the Asiatic species are now restricted to such sparse numbers scattered over such large areas that there is progressively less chance for males and females to encounter each other.

The so-called Pleistocene extinction, affecting large mammals almost exclusively, is still continuing more rapidly than ever before. Whatever was the case in the earlier stages, at the present time there is little question that man is the effective agent. Unless a really determined effort is made, there seems little hope that any except the black rhinoceros can be preserved into another century; and its extermination, although a little further off than the rest, also is probably inevitable. A specialist on the group naturally regrets their impending doom, but admittedly they are stupid animals, and, from a human view-point, antisocial. Perhaps, instead of deprecating their approaching extinction, we should recognize our good luck that a group, which might well have been exterminated in the prehistoric past, has survived to the present day to help interpret the remains of their fossil relatives and ancestors.

SECTION OF PSYCHOLOGY

JANUARY 20, 1941

PROFESSOR KIMBALL YOUNG, Queens College, Flushing, New York City; *A Preliminary Report on a Field Study of Farmers' Attitudes.*

This was a tentative report of a research project set up by the Division of Farm Population and Rural Welfare of the Bureau of Agricultural Economics, Department of Agriculture, designed to secure information on the attitudes and values of a selected sample of American farmers. The objective of the study as stated in the original plan was as follows: "To investigate the cultural, community, and social psychological factors in land use and rural life, with special reference to those factors which either facilitate or offer resistance to change, contribute to adjustments and maladjustments, and to stability and instability in the individual or community life." The planning and execution of this project was carried out by a group of experts in social psychology, rural sociology, and cultural anthropology. The full reports on this research will be published by the Bureau of Agricultural Economics.

In presenting his preliminary report, Dr. Young discussed chiefly the methodology with only a brief comment on some of the major findings. The completion of the investigation is still some months away.

SECTION OF ANTHROPOLOGY

JANUARY 20, 1941

DOCTOR GEZA RÓHEIM, New York, N. Y.: *Yaboaine, A War God of Normanby Island.* (No abstract has been received by the Editor.)

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 10 and 11, 1941

Conference on "*X-Ray Diffraction*"

The Section of Physics and Chemistry held a Conference on "X-Ray Diffraction," the second of the series this year. Doctors A. L. Patterson, Bryn Mawr College, and B. E. Warren, Massachusetts Institute of Technology, were in charge of this meeting as Conference Chairmen.

The program consisted of the following papers:

I. EXPERIMENTAL PROBLEMS INVOLVED IN THE COLLECTION OF DATA
  - I. 1. "Introductory Remarks on Apparatus and Techniques." B. E. Warren, Massachusetts Institute of Technology.
  - I. 2. "Production of Monochromatic Radiation." S. B. Hendricks, U. S. Dept. of Agriculture, Washington, D. C.
  - I. 3. "Diffraction Apparatus and Techniques." M. J. Buerger, Massachusetts Institute of Technology.
  - I. 4. "Measurement and Correction of Intensities." I. Fankuchen, Massachusetts Institute of Technology.

II. INTERPRETATION OF DATA
  - II. 1. "Introductory Remarks, Including Outline of Fourier Series Methods." A. L. Patterson, Bryn Mawr College.
  - II. 2. "Computation of 2-dimensional Fourier Series. G. Tunel, Geophysical Laboratory, Carnegie Institute, Washington, D. C.
  - II. 3. "Optical Synthesis of Fourier Series." M. L. Huggins, Eastman Kodak Company, Rochester, N. Y.
  - II. 4. "Diffraction Synthesis of Fourier Series." M. J. Buerger, Massachusetts Institute of Technology.
  - II. 5. "Interpretation of X-ray Diffraction Data by means of Fourier Series." D. Harker, The Johns Hopkins University.

III. DIFFUSE SCATTERING
  - III. 1. Introductory Remarks. P. Debye, Cornell University, Ithaca, N. Y.
  - III. 2. "Temperature Diffuse Scattering." W. H. Zachariasen, University of Chicago, Chicago, Illinois.
  - III. 3. "Diffuse Scattering of X-rays from Layer Lattices." S. B. Hendricks, U. S. Department of Argiculture, Washington, D. C.

IV. GENERAL DISCUSSION

Questions of organization and publication of diffraction results. M. L. Huggins and M. J. Buerger.

DOCTOR BARNUM BROWN, The American Museum of Natural History, New York, N. Y.: *The Methods of Walt Disney Productions.* (An extended abstract of an address given at the Annual Dinner and Meeting, December 11, 1940.)

Having seen a portion of FANTASIA, this audience will no doubt be interested in knowing something of the basic foundation on which this marvelous animated picture was created.

About three years ago, I had considerable correspondence with the Walt Disney Studios about dinosaurs, as to typical forms in the different geological periods, their history and relationship, the various kinds of life that could be associated with dinosaurs during the period of their existence, and the more primitive species that preceded the era of dinosaurs.

On the basis of these data, as well as models, restorations, etc., furnished through my efforts, a number of artists in the Walt Disney Studios proceeded to make scenes representing the dinosaurs and other typical associated reptiles. The plan was to run a continuous story from the beginning of life on this planet up to the close of the dinosaur era, which it was proposed to make the climax of the picture.

When a great many drawings had been made, I visited the Disney Studios and found their walls covered with very creditable and accurate types of prehistoric life including the associated floras. I made whatever constructive criticisms were necessary for the further development of the story. Naturally, there were many different groups involved in this story, and while material was not available for a continuous sequence, much of the prehistoric life was well represented.

In most of the scenes, the restorations of the creatures were generally not as exact as the scientist would require, but it should be borne in mind that in an animated picture of this kind it would have been impossible to have faithfully copied the accurately worked out material used in the studio as a control. Therefore, most of the creatures as now shown are necessarily conventionalized.

In the meantime, while different sequences were being created in drawings, Walt Disney, Deems Taylor, and Leopold Stokowski selected eight outstanding musical compositions that, in their opinion, could be coordinated with corresponding sections of the spectacle to best represent the action, after which the Philadelphia Symphony Orchestra played the entire program through, making a recording of 450,000 feet of film. This was cut to the 18,000 feet used during the time that the complete spectacle of FANTASIA is shown, but so skillfully that few, except experts, in any audience would realize that there has been any material loss in the compositions. In the Rite of Spring, by Igor Stravinsky, which was selected to accompany the theme embodied in the creation of the world and the evolution of the dinosaurs, only expert scientists would realize any major imperfections in the scenes as they progress on the screen.

Few of us, who have had the privilege of watching volcanic eruptions, can recall any volcanoes as active as those you have just seen, and yet the principles of vulcanism have been well shown, and, to most of the audience, the accompanying music has to a degree typified the effect.

Thus, Walt Disney has taken the findings of the scientist to show the culmination of dinosaur life through change of environment and their final extinction through their lack of capacity to adapt themselves to this type of life, or to seek, by migration, conditions that were favorable for the continuance of their existence.

These conditions were illustrated and emphasized by the results of one of our recent expeditions, during which the remains of a herd of dinosaurs were found in an ancient lake bed in the Big Horn Basin of Wyoming. Here, many dinosaurs had congregated in the fast receding lake bed, where they had all died in their tracks.

Several other incidents in this portion of FANTASIA harmonize with the findings of Museum expeditions.

In the presentation of FANTASIA, many new features of sound production and dispersal have been created and it will exemplify the remarkable progress of motion picture animation to date. Walt Disney and his more than 1200 associates, who at some time have worked on parts of this picture, are to be congratulated on a marvelous achievement in the picturization of interesting and fascinating features of animal life, accompanied by music that harmonized with the scenes portrayed. As some one has aptly said, "It more nearly represents hearing pictures and seeing music."

This Walt Disney production is a distinct contribution from an educational standpoint in the field of motion picture animation, showing as it does, the accepted course of major features of the development of the earth and the evolutionary trends of life during successive geologic periods, accompanied by appropriate classical musical compositions.

It is very gratifying to have returned from Texas in time to enjoy this presentation of The Rite of Spring from FANTASIA with the Academy and its guests this evening, and it is an especial pleasure to be able to show you some scenes from our recent field work, which resulted in much new information relating to our friends, the dinosaurs.

Last May, arrangements were made with the Sinclair Refining Company for financing an expedition for dinosaurs into the Big Bend area of Southern Texas, where dinosaurs were first reported by me in 1911. This fund was also to be used, in part, to cover transportation charges on the large series of fossil sauropod tracks excavated by Mr. R. T. Bird on a W. P. A. project in central Texas, which started in April.

Doctor Erich M. Schlaikjer, who was to assist me in the early part of the Big Bend work, left New York, July 30, for Rock Springs, Wyoming, where our truck was stored, and drove through to Glen Rose, Texas, where the tracks were being excavated, and I joined him there on August 8.

We proceeded to Marathon, Texas, making that town headquarters, and drove south 100–150 miles to the Cretaceous Bad

Land exposures that surround the Chisos Mountains. In 1939, Doctor Schlaikjer had located several specimens in this area, during a reconnaissance trip. We examined these first and then investigated different parts of the exposures for the purpose of determining in what area dinosaur remains were best preserved. It soon became evident that associated material was rare in the entire area and specimens worth excavating were rarer still, although fragments and incomplete dinosaur bones were found literally by the thousands, wherever there were good exposures.

The Aguaja formation in which Cretaceous dinosaurs are numerous is 220 feet in thickness in several measured exposures. These beds overlie the strictly marine Boquilla beds and, in turn, are comformably overlain by a series of Cretaceous clays 1600 feet thick, known as the Tornillo beds, in which only sauropod dinosaurs are found.

While Doctor Schlaikjer was with me, we covered the Aguaja beds east, south, and west of the Chisos Mountains, taking up several specimens and locating others that were excavated later.

In the meantime, Mr. Bird completed the excavation of the dinosaur tracks at Glen Rose and crated a part of them before joining me on September 10, when Doctor Schlaikjer returned to New York.

We completed the Big Bend dinosaur work, November 30, shipping the collection from Alpine and Marathon to Walnut Springs, the rail point for Glen Rose. We then took a short trip into Mexico at Presidio but found only marine exposures and no dinosaur-bearing beds south of the Rio Grande. From Presidio, we followed the Rio Grande northward on the Texas side in search of reported fossiliferous Tertiary strata.

North of Newman Springs, approximately 16 miles southeast of Porvenir, we found an extensive thick exposure of Tertiary strata thought to be Oligocene in age, covering a considerable area, in which a few bones and teeth of mammals were collected. Titanotheres, and also *Dinictia*, *Mesohippus* and *Oreodon*, have been found there. These exposures have been worked, though not exhausted, by the University of Oklahoma and are worthy of more extensive search.

A new chapter in our dinosaur work has been added as a result of the American Museum of Natural History—Sinclair Expedition to the Big Bend area. This is the most southern point in the United States where dinosaurs have been found. In all, eleven important specimens were excavated in this region, among which the following are the most noteworthy:

An incomplete but enormous crocodile skull having teeth three inches long and one inch in diameter (probably *Dinosuchus*), which was as large as a medium-sized dinosaur;

A disarticulated skull with associated limb-bones of a small ceratopsian, resembling and closely related to *Brachyceratops;*

Another incomplete ceratopsian skull, tentatively referred to *Pentaceratops;*

A ceratopsian pelvis, sacrum, and part of the dorsal series with ribs attached (probably *Pentaceratops*), which was the only articulated specimen found;

A complete skull of a new genus and species of a plated dinosaur resembling *Palaeoscincus.*

The dinosaurs collected by us and specimens seen in other collections at El Paso and the Oklahoma University represent a faunal facies comparable in age to that of the Judith River and the Mesa Verde Cretaceous of northern states, but most genera and probably all species are new to science. Two very large sauropod bones—a humerus and a cervical vertebra (*Alamosaurus*)—were excavated from the Tornillo beds, the vertebra being the largest cervical that has been recorded.

The sauropod dinosaurs found in the Tornillo beds show that climatic conditions favorable for the life of sauropods persisted in this southern region millions of years after that group of dinosaurs had become extinct in the northern states.

From the Palaeozoic and Mesozoic strata examined, many invertebrate specimens were collected, some being of unusual exhibition value.

Leaving Marathon, November 8, we drove 500 miles to Glen Rose, where the track specimens were crated and hauled to the railroad at Walnut Springs, the season's collection aggregating a carload of 44,000 pounds.

The rare sauropod track collection, made possible through W. P. A. assistance, has great value for our exhibition halls, for, with the trail of fore and hind feet displayed under the tail of the *Brontosaurus* skeleton already there, they will form a convincing record of the position and movement of the limbs of these creatures, when in motion,—our first definite evidence for the exact placement of the fore limbs in sauropod skeletons.

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SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 3, 1941

DOCTOR PAUL H. BIRD, Assistant Geologist, Board of Water Supply, New York, N. Y.: *Some Structural Features of the Hudson Valley.* (This lecture was illustrated by lantern slides.)

The tunnel of the Delaware Aqueduct passes beneath the Hudson River 5 miles north of Newburgh and traverses the broad Hudson Valley in a northwest-southeast direction at a depth below surface varying from about 600 feet to 1400 feet. The width of the valley along the tunnel line from Shawangunk Mountain on the northwest to the Highlands of the Hudson on the southeast is roughly 22 miles. It is underlain by three sedimentary formations, which are, beginning with the oldest: the Poughquag, the Wappinger and the Hudson River.

The Cambrian Poughquag consists of sandstone and conglomerate thoroughly cemented with quartz, and is commonly spoken of as a quartzite. Some beds are arkosic, but for the most part it is essentially a pure siliceous rock. Bedding is usually more prominent in the upper part than in the lower. The thick-

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Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

ness has been considered around 500 to 600 feet, but it is doubtful if it is more than half this in the vicinity of the tunnel line. Between it and the underlying Highlands Gneisses, there is a marked structural unconformity.

The Wappinger is a limestone formation of Cambro-Ordovician age that lies conformably above the Poughquag. The thickness is usually given as about 1000 feet, but tentative measurements in the tunnel indicate that this is considerably too small. Parts of the formation are essentially massive, while others are strongly bedded. In general, bedding is more prominent in the upper portion of the formation than in the lower.

The Hudson River formation, of Ordovician age, is made up of black carbonaceous shale and sandstone in greatly varying proportions. Data already secured from the tunnel indicates a thickness of well over 3000 feet. It is generally considered conformable with the Wappinger. For the most part, the bedding is sufficiently plain to indicate the structure, but there are occasional short stretches of tunnel where it cannot be seen at all.

All three of these formations were involved in both the Taconic and Appalachian mountain-building revolutions, which have resulted in a very complex set of structural features with a northeast-southwest trend. Structural studies based upon the limited number of surface outcrops in this region leave much to be desired, especially regarding the details.

The excavation for the aqueduct tunnel, which is about completed at the present time, has opened up a continuous section almost exactly normal to the structural trend of the country from one side of the valley to the other. During the driving of the tunnel, geologic inspection and studies were made only at critical places where features of interest to construction were concerned. Field notes for a continuous plan and section on a scale of 20 feet to the inch are now being taken, although only a few short stretches have been covered to date. The data that have been collected, therefore, consist for the most part of a mass of disconnected details.

Two major factors have played an important role in the type and intensity of deformation throughout the valley. In the first place, folding, faulting and accompanying minor structures are much more prominently developed along the southeastern side of the valley and gradually become less conspicuous to the northwest, due to the fact that the centers of the disturbances were somewhere adjacent to this side of the valley. In the second place, the relative competency of the various formations and members exerted an important structural control.

The northwest front of the Highlands of the Hudson consists of a series of spurs of the crystalline rocks protruding out into the sediments with embayments of the latter in the valleys between. Three such embayments are crossed by the tunnel line. The first to the southeast is Hortontown Valley, where several slivers of quartzite are exposed. The structure here is not at all clear, except for the fact that faulting is certainly present. The tunnel is entirely in the gneisses, and the effects of faulting at tunnel grade were of no practical consequence.

In the next valley to the northwest, known as East Fishkill Hook or Trout Creek Valley, outcrops are numerous and appeared to indicate, beyond reasonable doubt, a syncline of sediments pitching to the northeast with only minor faulting. The center of the trough has been partly eroded away and a prong of Poughquag quartzite extends up each side of the valley to the southwest about a thousand feet beyond where a boring along the northwest side showed it to be absent. Recently, the contractor sank a 12-inch drill hole along the southeast side of the valley about 700 feet from the previous boring. It was thought this boring, like the other one, would be entirely in the crystallines; in fact, there was little doubt. After passing through 60 feet of overburden it encountered 20 feet of quartzite, then 50 feet of Wappinger limestone, then back into more than a hundred feet of quartzite, before finally reaching the crystallines! Needless to say that the mental disturbance, resulting from this discovery, has been of greater magnitude than whatever it was that put that quartzite and limestone where it has no business to be! The solution is certain to involve a considerable proportion of guesswork, unless further data is obtained from the tunnel, which has not yet been driven through this stretch.

In the third embayment of sediments, known as West Fishkill Hook, quartzite outcrops along the tunnel line for about a mile, but apparently is not over 200 to 300 feet thick at any place. The tunnel here is about 600 feet deep and entirely in gneiss.

The most obvious structural break of this region is the fault, or series of faults, along which the northwest side of the Highlands has been shoved up over the sediments of the Hudson Valley. At the place where the tunnel crosses this structure, it is believed that the movement took place along two parallel faults about 200 feet apart. The one to the southeast is entirely in the gneisses at tunnel level and is marked by over a foot of gouge clay. The other, to the northwest, separates the gneisses from the Wappinger limestone and has developed only a couple of inches of gouge clay. This interpretation postulates a hidden block of quartzite representing the total thickness of the Poughquag formation between the tunnel and the surface, but there is room for it and some to spare.

After leaving the gneisses of the Highlands, the tunnel passes through a little over 9000 feet of Wappinger limestone. This block of limestone is folded, but the folds are open for the most part and the structure in general indicates a fair degree of competence. It is cut by many minor faults, none of which appear to represent appreciable displacement.

The most puzzling structure of the region, at least for the time being, is an inlier of gneiss adjacent to the above limestone block, on the northwest, and separated from it by a southeasterly dipping fault. A series of test borings about 1000 feet southwest of the tunnel line showed the gneiss to be about 500 feet wide at the surface, while, at the place where the tunnel penetrates it, it is almost exactly 1000 feet wide. The borings also showed that between the gneiss and overlying limestone there is about 35 feet, measured vertically, of Hudson River shale at a depth of about 200 feet, which appears to indicate that the limestone was thrust up over the gneiss and, in so-doing, was overturned, or over-rode a sliver of shale. It was hoped that the tunnel would provide further evidence bearing on the character of this fault-contact, but it entered the gneiss through an east-west cross fault, the south side of which is displaced to the west, instead of through the main fault, which strikes northeast-southwest. The tunnel, therefore, was put at the wrong place—for the geologist—just like the boring in Trout Creek Valley, but, in this case, it failed to settle an old problem instead of uncovering a new one.

The thrust fault along the northwest side of the gneiss inlier brings the gneiss against the Hudson River formation and, therefore, represents a displacement greater than the combined thickness of the Poughquag and Wappinger formations, which is probably close to 2000 feet. The Hudson River, adjacent to this fault, consists partly of shale and partly of sandstone. The shale is crushed to gouge clay for a distance of about 20 feet from the fault and the sandstone is broken into blocks, which are distributed at random through the shale for a considerably greater distance.

The Hudson River formation northwest from the gneiss inlier to the second block of limestone, beginning beneath the Hudson River, is isoclinally folded throughout and stands at a high angle. The bedding, slaty cleavage, and shearing are, all three, parallel through the most of this stretch of three miles. The shale portion of the formation deformed by flowage, while the sandstone fractured. At many places, angular blocks of sandstone, representing disrupted beds, are completely surrounded by shale. The shearing, thought to be the last step in the deformation, has produced remarkably well-developed slickensides. There are almost innumerable faults and other minor adjustments throughout, the details of which have not yet been worked out.

The second stretch of limestone, which is about 7000 feet wide in the tunnel, succeeds the shale beneath the Hudson River. The contact, which dips southeast about 30°, has always been considered conformable, but deformation, consisting of flowage, shearing, and crowding, at the place where it is penetrated by the tunnel, has obliterated the original relationship. The limestone near the contact is almost entirely recrystallized and streaks of it are sheared up into the shale. Also, at one place, a small mass of shale is sheared into the limestone. Flowage has destroyed the bedding structure in the lower several feet of the shale, but, where it can be seen closest to the contact, the dip is greater than that of the contact, and, at only a short distance away, it is almost vertical.

About a mile northeast of the tunnel, this strip of limestone splits into two branches with a wedge of the Hudson River shale between. The thrust fault that brings the limestone above the shale on the east side of this wedge extends into the limestone to the southwest and was cut by the tunnel about 2000 feet west of the Hudson River, where it had developed a crush zone 80 feet wide at a depth of 600 feet. To the southeast of this crush zone, the limestone is severely decayed and iron-stained for a distance of almost 1000 feet, although the bedding is not disturbed to any extent. Much of the decayed material could be excavated with a spade. The decay follows the bedding very sharply. In many places in the tunnel the rock is entirely sound immediately adjacent to beds that are completely decayed. The reason for this selectivity is probably compositional and studies are planned to determine, if possible, the details.

To the northwest, the tunnel passes again into the Hudson River formation. The thrust fault, situated a little over a mile west of the river, which forms the contact with the limestone, dips steeply to the northwest in the tunnel walls, but has a general dip of about 50° to the southeast. This apparent anomaly is believed to be due to the fact that the limestone broke into blocks with each succeeding higher block pushed further to the northwest.

From here to the northwestern side of Shawangunk Mountain, a distance of about 22 miles, the tunnel is continuously in the Hudson River formation, except for a stretch of 200 feet of Wappinger limestone approximately 2½ miles northwest of the

Hudson River crossing. This arch of limestone does not show on the surface at all, and probably doesn't extend above the tunnel more than a hundred feet or so.

From the thrust fault a little over a mile west of the Hudson River to Marlboro Mountain, roughly 4 miles west of the river on the tunnel line, the structure of the Hudson River formation is extremely complex. For the most part, the folding is isoclinal and slaty cleavage is well developed. Much of the rock is replete with shear and tension structures, which are filled with white crystalline carbonate and quartz, giving them a bold outline against the black and gray shale and sandstone.

Marlboro Mountain, rising to an elevation of somewhat over a thousand feet adjacent to the tunnel line, owes its presence to the fact that the Hudson River formation, from the surface to below tunnel depth at this place, is composed almost exclusively of massive sandstone. The original structural relationship of the sandstone to the rest of the formation has not yet been worked out in detail, although it is believed possible to do so from surface and tunnel data. At the present time, it appears that it represents a facies change from the shale both to the west and to the east. It also appears to be pretty well toward the top of the formation, if not at the top. The thickness is certainly well over a thousand feet at the center, and may be as much as two thousand feet. During deformation, it behaved as a competent member in comparison with the shale on both sides. In general, the structure is that of an asymmetrical syncline with low and very gradually increasing dips in the western limb, while the eastern limb dips steeply. The axis of the syncline, which is near the eastern side, is a sharp flexure that takes place in such a short distance that it could be photographed in the tunnel wall. The width of the syncline from rim to rim on the present surface is over two miles.

The lithologic character and structural habit of this part of the Hudson River formation are considered sufficiently distinctive to warrant a separate designation. In field parlance it has come to be called simply the "Marlboro sandstone," a term that suggests a separate formation, which does not appear to be justified.

However, the term "Marlboro member" is being used in field notes and, in all probability, will be used in the final geologic records of the tunnel.

An interesting and instructive incident occurred in connection with the structure of Marlboro Mountain. The flexure referred to above, which forms the axis of the Marlboro Mountain syncline, comes to the surface on top of the mountain in a valley several hundred feet wide with precipitous walls of broken sandstone over a hundred feet high. The dip in the easterly wall is about 70° west, while in the westerly wall, the bedding is about horizontal. The bottom of the valley is a swamp. The structure was interpreted as a thrust fault and a sharp lookout was maintained for its presence in the tunnel, but no fault showed up. The truth was not known until the tunnel walls were washed down preparatory to placing the concrete lining, and then it wasn't a fault any longer!

To the northwest of the sandstone of the Marlboro member, the tunnel passes through isoclinally dipping strata for a distance of about 3 miles. The dip varies from about 45° to 70° southeast. To consider this simply a continuation of the western limb of the Marlboro Mountain syncline would be to assume an immense thickness for the formation. It appears more probable that it is a series of closed folds, the one limb of each of which is overturned to the northwest. There is some, although not entirely satisfactory, evidence in the tunnel indicating this type of structure. Specimens are being collected for the purpose of making a study of grain size gradation to determine the top of the beds through this stretch.

Northwest of the isoclinally dipping strata, the folds are open and the dips gradually decrease until beneath Shawangunk Mountain there are only one or two folds with dips over 10° to 15°, and they are near the eastern front. Along with the decreasing dip of the bedding, the slaty cleavage gradually becomes less prominent and does not show up at all in the tunnel to the northwest of the Wallkill River, although, at the surface, it has been observed in a few outcrops within a mile of the base of Shawangunk Mountain. For a distance of at least 10 miles northwest of Marlboro Mountain, there are stretches where shear and tension structures are prominently developed, and, in some places, are so complex as to almost defy any dependable interpretation.

The fact that this region has been subjected to two periods of compressional deformation has been mentioned. In addition, there is a possibility that it was also affected by Triassic block faulting. Efforts to differentiate the structures that may have been produced by these three periods of deformation on the basis of evidence furnished by surface outcrops, have not proved very fruitful. Accordingly, this problem has been kept constantly in mind. To the present time, no evidence whatever has been noted that may have a bearing on the age of the folding, but there does appear to be some ground for distinguishing different ages, or at least different types, of faulting.

In a stretch of about two miles of tunnel in the vicinity of the Wallkill River, faults are numerous and appear to be of three types. The first type, which is tentatively considered the earliest, always dips steeply; is accompanied by sharp, prominent drag folding; and shows no indication of crushing. These faults are often about parallel to the slaty cleavage where it is present.

The second type is invariably accompanied by crush zones consisting of comminuted rock and a mixture of crystalline carbonate and quartz. The dips vary through a great range and show a decided tendency to parallel the bedding. The slaty cleavage shows drag where this type of fault cuts it at an angle, although the drag, whether in cleavage or bedding, is not nearly so pronounced as in the first type. In many instances, tension fractures are associated with this type. Both the first and second types are thrust faults and dip to the southeast, with the displacement of the hanging wall toward the northwest. Several instances have been noted of the second type dipping gently to the northwest, with the hanging wall displaced in the same direction. In one case, a fault of the second type had displaced one of the first type.

The third type is always nearly vertical, and, in the few instances where drag is present, appears to consist of normal faults. They are often accompanied by prominent crush zones which are filled with gouge clay and which carry no crystalline carbonate or quartz. One such crush zone is over 30 feet wide. The southeast side is always displaced down with respect to the northwest, where the direction of movement can be determined. On both sides, there is usually a well-developed set of joints parallel to these faults.

SECTION OF BIOLOGY

FEBRUARY 10, 1941

DOCTOR H. J. MULLER, University of Edinburgh and Amherst College, Amherst, Mass.: *The Threads that Weave Evolution.* (This lecture was illustrated by lantern slides.)

All the great body of work in genetics goes to establish very firmly the conclusion that the threads of nucleoprotein known as "chromosomes" form the material basis of nearly all the hereditary characters differentiating individuals of the same species from one another—the only certain exception being, in plants, characters dependent upon the chloroplastids. Nevertheless some biologists still dispute the conclusion that the differences between the species themselves, and between still more widely separated groups, are mainly determined by the chromosomes. Hence, they refuse to regard the chromosomes as the material basis of evolution, in the larger sense. Jaques Loeb, for instance, maintained that the nature of the "embryo in the rough" was ultimately determined by the cytoplasm, and assigned to the nucleus only the task of putting on the finishing touches. And the view has been recently revived that, while geographical races may be distinguished by Mendelian (hence, chromosomal) differences, which arose by mutation, the more fundamental differences between "good species" (i. e., species that cannot cross, or whose hybrids show by their sterility some deep-seated incompatibility of germ plasms) are of a quite different nature.

Reasoning "by elimination," it seems logical to suppose, with Darwin, that the change from one species to another represents merely an accumulation of these heritable variations which are found to differentiate one individual from another, just as it is logical to suppose, with Lyell, that the great geological changes of the earth represent an accumulation of the small changes visible on its surface in a single life-time. But the individual variations have been found to be due to mutations in the chromosomes (and to a lesser extent, in plants, in the chloroplastids).

If there were some other process of species formation than by accumulation of such mutational steps, the systematic studies of an army of geneticists, carried out over four decades, should, by this time, have gathered some evidence of it in their own observations of the occurrence of variations. The only large jumps observed from one "species" to another have been the origination of multiple sets of chromosomes ("polyploidy")—a process itself chromosomal in basis but largely confined to plants and obviously of very limited occurrence in past evolution,—and the origination of combination-types through crosses of preëxisting species—a process subject to similar limitations and requiring differentiation of species from each other by some more primary method in the first place. The only other escape, besides large jumps, from the conclusion that species originate, broadly speaking, by accumulation of Mendelian differences, is to go to the other extreme, and to suppose that there is a very gradual process of variation, occurring simultaneously and practically equally in all the members of the population, so as to transform it, in the course of an extremely long period, into a different species without meanwhile causing individual differences between the members of the population that are present at any one time. Not only is there no evidence for this special hypothesis, but it would encounter the difficulty that, in this case, two gradually diverging populations should, before reaching the species level of divergence, show the clearly non-Mendelian differences here postulated.

It will be seen that the above "argument by elimination" is based on the negative proposition that non-chromosomal individual differences in heredity do not exist, outside of chloroplastids. Any such negative proposition is difficult of rigorous universalization and hence should be supplemented by positive evidence. Such evidence of the chromosomal nature of the differences between species has been obtained in a number of ways. For one thing, undoubted cases of Mendelian differences between species have been obtained in some crosses; for example: in snapdragons by Baur, in cotton by Harland, in guinea pigs by Detlefson, and in the striking work on blood differences between pigeons and doves by Cole and Irwin. The objectors to a thoroughgoing chromosomal view, confronted with such evidence, retreat to the position that, while some Mendelian differences between species do exist, that is only a part—in their view, a minor part—of the story. They would still maintain that the more fundamental differences, including especially those which cause the deep-seated species incompatibilities, such as the inability of the primary germ cells in a species-hybrid, like the mule, to undergo even the preliminary processes characteristic of the growth period, are referable to discrepancies inherent in their protoplasms. Standing in opposition to this, cases like Kosswig's and Gordon's killifish hybrids and Hollingshead's cereals, in which a Mendelizing gene of one species has been demonstrated to give rise to pathological effects, when placed in the genetic setting of the hybrid, illustrate how chromosome differences can cause such incompatibilities, but, as yet, the non-geneticists give these the status of special cases. Meanwhile, a more general analysis of species differences that might demonstrate the correctness of the chromosomal view has been hampered by two circumstances: (1) the fact that when "good" species are defined as those which are uncrossable or which give hybrids showing a deep-seated sterility, analysis of their differences by ordinary genetic methods is precluded by this very unmixability; (2) were it possible to obtain later generations from such a species cross, the gene-differences would be expected to be so numerous (as is often the case even in crosses between sub-species) as to obscure the segregation of the individual genes concerned and so to make a solution in terms of genes or chromosomes very difficult.

It is therefore desirable, for the genetic analysis of species differences, to have recourse to methods different from the orthodox ones, methods that in one way or another avoid or overleap the barrier of hybrid sterility and the mire of "multiple factors." Several such methods have been applied, with concordant results.

One such line of attack lies in the old studies of Herbst, Boveri, and others on the larval hybrids (plutei) between different species, in fact, different genera, of sea urchins (notably *Sphae-*

rechinus and *Paracentrotus*). Here it was found that, whereas the ordinary hybrids were intermediate in type, hybrids or even parts of hybrids that had received more than one set of chromosomes from the mother genus and only one set from the father, and so having abnormally large nuclei, resembled the mother genus more nearly, regardless of whether or not they had also received more cytoplasm from the mother. As a further check, it was found that larval parts of the hybrids which had received one set of chromosomes from the mother and none from the father genus, and so had abnormally small nuclei, were likewise of the mother's type. All this made it quite evident that these genus differences were dependent upon the chromosomes and not upon the cytoplasm.

In the green alga, *Acetabularia*, decisive evidence of the nuclear determination of species differences has been obtained by Hämmerling. This alga is shaped somewhat like a minute palm tree and, although it is 1 to 5 cm. high, it has all of its chromatin localized in a large nucleus in the basal rhizoid. There are two morphologically very different species of it, *mediterranea* and *Wettsteini*. When immature plants are cut in two transversely, just above the nucleus-containing region, it is possible to graft the upper (anuclear) portion from one plant on to the lower (nuclear) portion of another. When the lower part is of the *mediterranea* species and upper *Wettsteini*, then when the grafted *Wettsteini* part grows further, given enough time, it eventually forms a *mediterranea* crown while, *mutatis mutandis*, an upper *mediterranea* stem on a *Wettsteini* base forms a *Wettsteini* crown. Hence, the species morphology clearly depends on substances emanating from the nucleus below, which direct the type of growth and differentiation of the engrafted cytoplasm. At the same time, however, the chloroplastids of each species remain self-perpetuating, and disharmonies finally arise from their interaction with products of the nucleus of the foreign species.

In higher plants, where the ordinary hybrids between two species are capable of breeding at all, the descendants—the second and later generation hybrids—show a greater range of variation than the parents, and it is possible eventually to obtain individuals resembling either parent species in respect to any chosen characteristic, although the genetic differences may be too complex to allow an analysis in terms of individual genes. That these results are due to the segregation of chromosomal genes is, however, made evident by the contrast between these cases and another great series of plant species hybrids, in which the ordinary hybrid is sterile but the hybrid with a doubled number of chromosomes (i. e., with two sets of chromosomes from each species and, hence, with four sets in all—a "tetraploid") is fertile. For in these fertile hybrids—in which each chromosome has a mate like itself, derived from the same species, and in which there is, therefore, no opportunity for segregation of chromosomes of the two species from one another—the above mentioned greater range of variation in the later generations does not occur. They breed virtually true. Thus, the segregation of the species characters must depend on the segregation of chromosomes, i. e., these characters are chromosomally determined. Moreover, the "sterility" in the first case evidently consists in an inviability of gametes and/or young zygotes, caused by the formation of incompatible chromosome combinations by Mendelian recombination.

It is evident that such experiments cannot deal with cases where the hybrid "sterility" is more deep-seated than this—consisting in a real inability to undergo the maturation processes, as in many animal hybrids. Hence, this evidence might not be regarded as fully demonstrative by those objectors who maintain that the more fundamental evolutionary steps are to be found principally in the origination of groups that show a real hybrid sterility (consisting in the inability to form gametes at all) or an uncrossability of the parent species. Such an enforced retreat as this, however, is distinctly disadvantageous to their argument. On the other hand, it should also be noted that, on account of the presence of chloroplastids in plants, it is not to be expected that determination of species differences by their chromosomes will be complete or universal. In accordance with this, differences between reciprocal species-crosses are found in some plants. In these cases, there is clear evidence for the persistence in the cytoplasm, throughout generation after generation of back-crosses to males of a given species, of the genetic basis of characters introduced from the original mother-species. That these characters are really due to the chloroplastids rather than to other cytoplasmic elements is difficult to prove, and the peculiar conditions necessary for the proof have not yet been realized. Hence, the most decisive evidence for our thesis is, for the present, to be looked for in animals rather than in plants.

Turning to animals, a case similar to that of the plant polyploids above was long ago reported in moths by the Finnish geneticist, Federley. In *Drosophila*, a detailed analysis has been made by Dobzhansky of the results of crossing two "races," A and B, of *Drosophila pseudo-obscura*, whose hybrids in one sex (the female) are very infertile and, in the other sex, quite sterile. He used the well-known method of determining which chromosomes were present in later generations, derived from the infertile hybrid crossed to the pure species, by means of the appearance or non-appearance, in the individuals examined, of given mutant characters ("markers") of known chromosomal location, that had been present to begin with in the chosen individuals of one of the races. By this means, it was shown that the sterility and infertility were dependent upon the presence of a mixture of chromosomes. Genes tending to cause hybrid infertility were present in all the chromosomes, and only those descendants having all chromosomes of both their sets derived from one race only were fully fertile.

It would seem a quibble to object that the above experiment dealt with "races" rather than good species, for, in cases like this, there is no empirical ground for drawing a clear-cut distinction between race differences and species differences. However, in the following case, recently worked out in Edinburgh by Pontecorvo and the present author, with amplifications by Koller and Słizynski, not even such an objection can be made. For the two groups concerned—*Drosophila melanogaster* and *Drosophila simulans*—are generally conceded to be "good species," even though morphologically similar, and their hybrids of both sexes are completely sterile, owing to an inability of the primary germ cells of either sex to even commence the growth period and maturation. This case is one in which special means were used to circumvent the first-generation hybrid sterility, by obtaining, in the very first generation, types which had chromosome compositions like those which would have been expected for the second generation, had the first generation been able to breed.

The above object was accomplished by using as the *melanogaster* parents triploid females, which, like triploids in general, produced gametes containing one or more extra chromosomes besides one complete set of chromosomes. Mutant genes for known recessive characters were provided in all the chromosomes of these triploids. The triploids were mated to normal *simulans* males that had been subjected to heavy X-raying—a procedure which we had found to result in the eventual loss, from the offspring formed, of individual chromosomes that had been present in the treated sperm. When a sperm lacking a given chromosome happened to fertilize an egg having the corresponding chromosome represented twice, a viable offspring was produced in which, as in a putative second generation hybrid, this particular type of chromosome was derived entirely from the mother species, while, at the same time, the other chromosomes were "hybrid" (having one of their two members from each species). Which kind of chromosomes had been so affected could be ascertained by noting which recessive mutant character of the mother was manifest in such an individual. In this way, it was found that the death of certain types of the hybrids was dependent upon their having a given mixed chromosome composition. Moreover, the morphological abnormalities (missing bristles, etc.) of other classes of the hybrids likewise depended on the presence together, of particular chromosomes from the two species. And, as for the hybrid sterility, all viable mixtures, in which any major chromosomes from both species were present together, showed sterility of one or another grade.

The conclusion that the sterility just mentioned was wholly due to the chromosomal make-up of these individuals, was given a complete verification by the occurrence of a single fly that had received all its major chromosomes from one species only (*melanogaster*), for this hybrid alone was found to be fertile. By breeding it to *melanogaster*, its minor chromosomes were transferred into populations that were otherwise purely *melanogaster*. A study of their characteristics, as shown under these conditions, proved that the minor chromosomes in question (the Y and the fourth) differed slightly from the corresponding *melanogaster* chromosomes in regard to the potencies of various genes. It is believed that these slight gene-differences illustrate the kind of change which, carried further, have resulted also in the hybrid sterility and in the other kinds of incompatibilities found in species crosses. At any rate, all the incompatibilities studied in the present experiment have been demonstrated to be of chromosomal origin.

The various lines of evidence recounted above, when taken together, make out a very strong case for the extension of our conclusions regarding the nature of the differences within a species to the differences between species, and so justify our designation of the chromosomes as the material basis not merely of heredity, in the narrower sense, but also of evolution, in general.

Concerning what happens within a chromosome when one of these differences arises, i. e. when a "mutation" occurs, a number of facts have been discovered. It has been found convenient to divide mutations in chromosomes into gross and minute structural changes and "gene mutations." Analysis of the gross changes produced by radiation has shown that these arise by breakage of the chromosomes at two or more points, followed by adhesion of the broken ends in a new linear order. The minute rearrangements arise in substantially the same way, except that the two breaks involved in their production are usually produced by a common cause (ionization or activation), instead of by independent accidents. As for the "gene mutations," it is not unlikely that they represent a still minuter type of rearrangement of material, but it would be rash to maintain that the only difference between them and the above "minute changes" lies in their smaller size.

The peculiar thing about the chromosomes, which enables them to function as the material of evolution, is that, after these changes have been produced in them, when they reproduce, the daughter chromosome that is formed constitutes a faithful copy, not of the original normal chromosome, but of the newly changed pattern. That is, the existing pattern, whatever form it may have, serves as a mold or model according to which the daughter pattern is constructed. Thus, an endless number of different changes in pattern and higher combinations of these can be tried out in evolution. Extrapolating our conclusions regarding the basis of evolution to lower organisms, we would conclude that the differences between them also depend upon their chromosome-material. That is, their different protoplasms are to be considered as by-products of their chromosomes (genes). Thus, the latter would represent the primary material of life and the first essential constituent of the lowest forms of living matter, as the author pointed out in 1921 and 1926, when discussing viruses as representing relatively naked genes. This conclusion has recently received striking support from the work of Stanley, showing that tobacco mosaic and other viruses may be obtained, without loss of their potentiality of reproduction, as crystalline nucleoproteins. In other words, they consist of material having essentially the composition of chromosomes and apparently of nothing else. This finding, then, helps to justify us further in our contention that the chromosomes are the threads that weave biological evolution.

SECTION OF PSYCHOLOGY

February 17, 1941

Mr. Paul F. Lazarsfeld, Director, Office of Radio Research, Columbia University: *Studying the Effect of Radio.*

There has been much discussion about the changes which radio has brought, or will bring about, in American life. But few actual data are available on this point. The reason is partly that, in spite of its spectacular rise, the history of radio is still very young, and partly because studying the effect of social institutions is a very difficult task. This paper summarizes the various techniques which have been used so far to study the effect of radio.

To this end, it is necessary to state more precisely what is meant by "effect." The word has really at least four different meanings, and they can be expressed as follows:

(a) *Pre-selective effects.* It is obvious that the ultimate effect of a program depends upon whom it reaches. Radio cannot educate the broad masses of the population, if they do not listen to educational programs. An appeal for tolerance cannot have any effect, if intolerant people shy away from programs which teach tolerance. Therefore, the first step in an effect-study is to find out the audiences which the different programs attract.

(*b*) *Liked and disliked elements in programs.* Even if people are willing to listen to a program type, much of the effect will still depend upon how well the program is constructed in all its details. Therefore, methods have to be devised to find out what people like and dislike, specifically, in a given program.

(*c*) Once people are willing to try a program, and once they like it enough to continue listening, then effect-studies in the narrower sense can be carried through. One type to be studied is *short-range effects:* Does the program make people read, buy, vote, etc.?

(d) *Long-range effects.* Finally, we will be interested, and, probably, most interested in what one might call the institutional changes which radio brings about in society. Does it increase interest in good music all over the country? Does it endanger the future of the newspaper? And so on.

Any exhaustive study of the effect of radio has to take these four types of effects into account. They can be visualized along a time-line, the pre-selective effects starting before the program is even listened to, and the long-range effects lying at the far end of the entire development. We shall now present a few examples of how such effects are measured and what has been found so far.

(1) The structure of the audience to different types of programs is, by now, well-known, and the differences are considerable. Programs, such as the Philharmonic Symphony, and "Information, Please!" are listened to relatively most frequently by upper-class people. On the other hand, programs which give advice on marital problems, cooking, and the like, have their peak-audience in low income groups. Jazz music is listened to mainly by people under thirty-five years of age; whereas religious programs are most attractive to older people. Certain leading comedians are rather universal in their appeal. Information, such as this, is gained from continual surveys of the population, which are conducted by many commercial agencies. The techniques of audience surveys have been well developed. The proportion of the population listening to a certain program is known as the "rating" of the program.

(2) Detailed program research, with the purpose of finding out what is liked and what is disliked about the program, has made much slower progress. This reporter, in collaboration with Dr. Frank Stanton of the Columbia Broadcasting System, has developed a mechanical device called the program-analyzer, as one means of discovering more precisely the portions of a program which are liked and disliked, The listener holds in his hands one green and one red button. These buttons are electrically connected with a moving tape which runs through the machine and which, in turn, is synchronized with the phonograph playing the recorded disc to which they are listening. As the listener approves or disapproves of any part of the program at any time

during its performance, he presses the green or the red button to show his like or dislike, respectively. This reaction is recorded by pens on the moving tape for each listener. When the program is over, it is possible to read the tape and thus to learn which part of the program each listener liked or disliked. When the program is played back, the listener can be asked, at each critical point, just why he liked or disliked the program at that juncture. By thus separating the mere recording of the reaction and the introspective analysis, we do not, on the one hand, break up the experience of the listener, and, on the other hand, we also keep him from forgetting what he liked and what he disliked. In this way, we secure very rich information on the effect the program produces on him, at the time of his listening. Programs which have been improved with the help of this device were liked afterwards better than they had been before.

(3) The short-range effects of radio are tested frequently by the advertisers who pay for the radio programs, since they are eager to find out whether they are selling their goods. Studies have been made of the sales of a product in cities which carry a program advertising it, and in comparable cities where the program is not on the air. Invariably, the cities with the program sell more of the advertised merchandise. It has also been shown that, where there are book programs on the air, the books discussed have greater circulation in the local libraries. The techniques of studying these short-range effects are being steadily improved. One of the latest developments along these lines has been to interview people repeatedly. During the last presidential campaign, for example, a sample of citizens was asked each month what speeches they had listened to, and what candidates they were likely to vote for, at the time of the interview. The changes in political opinion were related to the actual listening habits of these people. By and large, it can be said that radio propaganda is most successful in the commercial field and relatively less successful in the political field, because the carefully maintained neutrality of radio balances the effects of the two parties struggling for power in a typical American campaign. The very great

potential propaganda power of radio would come to the fore if it were monopolized by one side of a debate. As long as both sides have access to the radio, people usually listen, for the most part, to the side with which they agree and, therefore, the effects are minimized.

(4) What radio will do in the long run, for obvious reasons, is most difficult to predict today, partly because the effects might be very indirect. For example, it is safe to say that radio has not made people read newspapers less. Quite the contrary, by broadcasting so much news, it has probably also increased interest in news reading. At the same time, however, it has taken much advertising money away from the newspapers, and, therefore, it will probably bring about many changes and restrictions in the newspaper field, in spite of the fact that it does not reduce newspaper reading. Without doubt, radio has also greatly increased the interest in good music. As far as the system of public instruction goes, radio will probably make a great change, but it is only being accepted very slowly by schools as a legitimate help in teaching.

In appraising the future effects of radio, one has to keep in mind that a person who becomes interested in public affairs or in good music through the radio, has a different, and probably a more superficial attitude toward his new interest, than people who have been initiated to the same subjects by their immediate personal environment. Thus, there are two tendencies implied in the future of radio. One is that large masses of people will have the opportunity to participate in matters from which they have been excluded up to the present time. The other is that this participation is a looser and less stable one. Whether the future effect of radio, therefore, will be fortunate or unfortunate, depends very much upon the wisdom with which it is handled by the population, in general, as well as by educational, business, and governmental agencies.

SECTION OF ANTHROPOLOGY

January 20, 1941

Doctor Geza Róheim, New York, N. Y.: *Yaboaine, A War God of Normanby Island.*[1]

Yaboaine is the sky-god of Normanby Island. He appears in the role of a High God, at present, though, undoubtedly, after some confusion with Yaubada, the missionary rendering of the Biblical God (Jehovah). The narrative that figures most prominently in this connection is that of the Garden of Eden. The reason for this is obvious. Both the garden and the theft are emotionally significant things for the native, and Yaboaine is especially closely connected with theft and with punishing theft. According to Bromilow, in Dobu there are two Yaboaines. One of them is the creator; the other the patron of thieves.[2] In my Duau area, Yaboaine is rather the god that punishes thieves than the patron of thieves. He is also and, quite clearly, the voice of conscience—the *Super-Ego.* When the barau (sorcerer) is trying to kill somebody by an incantation, he first chants another incantation to close the victim's mouth and anus, so that his voice, when he calls people to help him against the sorcerers, can not come out. In consequence of this incantation, when the victim calls out for help, the people don't hear him. Yaboaine, in the sky, hears him and shouts down to the kasanumu (underworld place, the spirits of the soil) to prevent the crime. They hear Yaboaine's voice and they shout to the people, but the people don't hear what they are shouting, and so the voice of conscience has failed to assert itself.

But Yaboaine is not merely a mythological personage. He is deeply embedded in native life and ritual. When the natives have caught a pig, they will shout up to Yaboaine, "Take this miserable pig up for yourself, and send us better ones instead, right into the middle of our net. *Uu uu uu!*"—with a long drawn out call. The same "shouting up" occurs in all other moments at which native life reaches a climax. When they are launching a canoe, or when they have piled up the yams in the tower-like structure of the *siwapa* at the feast, they will shout up to Yaboaine, "This is really nothing what we have here. Take it up into the sky and send down one that is really loaded," or something of this kind. Especially, however, they will shout up to Yaboaine when the triumphant war party returns with a captive for the cannibal meal, "These are but weak warriors. Take them up into the sky and send men with the courage of eagles, instead." At first sight, this looks like humility, "We alone can not do much. Victory is thine, O Lord." However, we have already shown that the shouting up to Yaboaine is connected with the triumphal phases of life. Mourning, for instance, the period of *mwamwadu*, characterized by a reduction of oral activities (inferior food, no shouting, noisy talk, etc.), ends with a pig hunt, and, when the pig is caught, they "shout it up" to Yaboaine. Here we can see quite clearly what happens. A period of melancholia (the Ego bowed down under the reproaches of the Super-Ego) is ended, and, in the great deed, the achievement, in the mania phase, Ego and Super-Ego are united. If Yaboaine is the Super-Ego, the simple explanation would be that, at this phase, the Super-Ego above has to be appeased, and they are doing this by offering what they have—the yams, the pig, the war-captive.

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[1] This paper, here abstracted in full, was announced only by title in Transactions, Ser. II. 3, 3. Feb., 1941.

[2] W. E. Bromilow, *Twenty Years among Primitive Papuans.* p. 87. 1929.

But if we know the life and the customs of the people of Normanby Island, we soon notice that this shouting up to Yaboaine is only one instance that conforms to a general pattern. This well-nigh universal formula, the true driving force behind most of their economic efforts, is the formula of refuted reproaches. At a *sagari* or *mwadare* (food distribution ceremonies between two clans), we find the following situation: One group, those who are giving the yams, shouts towards the other group, "I am lazy and don't make a mwadare!" or "There are no chiefs in Gagayowana, only little children," or "I worked in your garden even when the sun was shining, and you don't think of making a mwadare." These are the critical remarks made by the other group. They are now triumphantly refuted by the food distribution ceremony. This is exactly what the Super-Ego is, namely, the reproachful voice of the parents which we have introduced, transformed into a part our personality-structure. When we manage to refute these reproaches or this criticism by our achievements (or in phantasy) we are in the manic, euphoric mood of the feast-giver at the feast of the warrior who returns triumphant from war, and shouts up to the sky a projection of the Super-Ego, "So I am no good! I am not a real warrior! That's what you think! Look at the captive as proof of my victory!" So far, the situation seems to be quite clear. However, there is another aspect of Yaboaine which we have not taken into consideration. In some of the myths, Yaboaine appears as a woman. In one of the genuine witch incantations obtained from Donigo, Yaboaine in the esoteric love of the witches appears as being alternately a man and a woman. When Yaboaine appears as a woman, she is the leader of witches and, in both cases, she is on the side of the powers of destruction. This would seem to explain a peculiar feature in the ceremonial of the returning war party. For the man who leads the victim by the hand, wears a skirt. Now, the victim, the one who is to be roasted and eaten at the cannibal feast, is obviously closely connected with Yaboaine, because his spirit goes up into the sky to Yaboaine[3] or, as they say, "He became a Yaboaine." One would, therefore, assume that the captor with his skirt is identifying himself not with a god, but with the war-goddess, Yaboaine.

Here again we see how difficult it is to interpret without being personally familiar with the situation. This is a plausible explanation, yet we shall see that it is wrong. Yaboaine has a sister called Sine Mata Nogi Nogi (Woman Eyes Hard) or Silili. Sometimes this woman is not Yaboaine's sister, but identical with Yaboaine. She is, in both cases, the leader of the man-eating witches.

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[3] While other spirits go to dwell on Mount Bwebweso.

The interesting thing is that "Sílili" means something. It was explained to me as the "Inciter." For instance, you would say to a dog "sílili," i. e. "catch him." Sílili is the spirit that makes us do wicked things such as *loyawe* (love-making) war, or theft. Therefore, Sílili and Yaboaine must be closely related to each other, as the *Id* is to the *Super-Ego.* More than that, however, we are told that when the war-party starts, they say to each other, "Go and catch him," i. e. "Sílili." If one of these supernatural beings, who are so closely related to each other, is derived from a sentence or from the emotion of a moment (cf. Usener's "Augenblicksgötter"), it would seem natural to assume the same origin for the other. "Sílili" they say, when the war party goes out, and the Yaboaine ceremony occurs, when the party returns. Yaboaine also has a meaning. There can be no doubt that it means "Ya bo wahine"—"I am a woman." When they go out, they say "Catch him," and, when they return, "I ám a woman," denying their aggressions under the influence of their guilt feelings. This would be the same thing as the well known ceremonies to expell the ghost that threatens the members of the returning war party.[4] But we have the "shouting up" part of the Yaboaine complex, and this is closely connected with the *refuted reproaches* formula. We must re-interpret the sentence and say, "I am a woman" does not mean "I am a woman," but it is a refuted reproach,—"So you think I am a woman." "Now look for yourself! I am the victorious warrior!" In fact, there is one little ceremony in connection with the war party which confirms this interpretation. One man shouts to the other who wears the skirt "Why have you got a skirt on?" and he replies, "It is only a play skirt"; i. e., "I am only playing at being a woman, that is precisely what I am not."[5]

But, as we have said before, Yaboaine-Sílili is also the patron goddess—the leader of all witches. These corpse-eating witches, who are initiated in a phantasied rebirth from their mother, are clearly representatives of the anxiety-aspect of infantile body-

[4] Cf. J. G. Frazer., *Taboo and the Perils of the Soul.* 1911. 165–190.

[5] Cf. for similar rites, Bateson, *Naven.* 1934.

destruction phantasies (M. Klein. For the proofs for this statement I must refer to forthcoming publications). The child has the phantasy of tearing all the good things out of the mother's body, and the corresponding *talio* or retribution-anxiety about what the "bad mother," the witch, will do to the child. There is one aspect of the Yaboaine beliefs which we have not mentioned. Yaboaine is "our maker"; but not in the Christian sense of "Creator." He is pre-eminently the God that forms the embryo in the womb. A body-destruction phantasy is that of tearing the embryo, the sibling rival, out of the womb. This is represented by the witch, who opens the grave, takes the corpse out, and re-animates it or eats it. The reparation aspect of the same phantasy is Yaboaine, who shapes and protects the embryo in the womb. And what is the Super-Ego? Not only the introduced aggression of environment, but, in its even more archaic phase, our own aggression against environment turned inwards against the Ego, and represented by the opposite.

Although I have given a philological explanation of Yaboaine as the name of a god, which is really in keeping with the traditions of Max Muller, and, although I have derived this god (or goddess) from a ritual, yet behind this situation-conditioned origin of an "Augenblicksgott," we see, revealed, the whole structure of the human *psyche* with its interrelation of Id and Super-Ego of archaic destruction and reparation phantasies.

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 14 and 15, 1941

Conference on "*Viscosity.*"

The Section of Physics and Chemistry held a Conference on "Viscosity," the third of the series this year. Doctor H. Mark, Department of Chemistry, Brooklyn Polytechnic Institute, was in charge of this meeting as Conference Chairman.

The program consisted of the following papers:

"Introductory Remarks," by H. Mark.

"Forces and Molecular Arrangement in Liquids," by P. Debye, Cornell University.

"Viscosity of Liquids," by H. Eyring, R. E. Powell and Ch. Clark, Princeton University.

"Distribution Functions in Condensed Systems," by J. E. Mayer, Columbia University.

"Viscosity of Large Molecules in Solution," by R. Simha, Columbia University.

"Experimental Aspects of the Viscosity, Diffusion and Sedimentation of High Polymers," by E. V. Kraemer, Biochemical Research Foundation, Newark, Delaware.

THE NEW YORK ACADEMY OF SCIENCES

announces

TWO PRIZE CONTESTS FOR 1941

I

THE A. CRESSY MORRISON PRIZES

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1941, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

II

THE GEORGE FREDERICK KUNZ PRIZE IN GEOLOGY AND MINERALOGY

In accordance with the terms of a bequest by the late George Frederick Kunz, the accumulated annual income of the sum of $1,000. will be offered, at intervals, by the Council of the Academy for the best paper in the field of Geology and Mineralogy. This prize, amounting to $200., is offered for the year 1941, for the first time, and will be awarded at the Annual Dinner in December.

CONDITIONS:

(1) Eligibility. For Contest I. Membership in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

For Contest II. Open only to members of The New York Academy of Sciences, in good standing prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1941, to the Executive Secretary of The New York Academy

of Sciences at The American Museum of Natural History, Central Park West at 79th Street, New York City.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize of prizes will be omitted for these contests.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes in Contest I shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1941 by The New York Academy of Sciences."

Wherever and whenever published, the paper awarded the prize in Contest II shall be accompanied by the statement: "Awarded The George Frederick Kunz Prize in Geology and Mineralogy in 1941 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,
Central Park West at 79th Street
New York City

DUNCAN A. MACINNES,
Recording Secretary

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 3 May, 1941 No. 7

SECTION OF GEOLOGY AND MINERALOGY

April 7, 1941

Professor Ernst Cloos, The Johns Hopkins University, Baltimore, Md.; *Flowage and Cleavage in Appalachian Folding.* (This lecture was illustrated by lantern slides.)

Quantitative determinations of the amount of deformation due to folding are rarely successful because of the lack of reference values on which measurements can be based. Bedding planes can be assumed to have been more or less horizontal before folding. The amount of lateral shortening of beds due to pure folding can, therefore, be measured, and is closely linked to proportional thickening and the accumulation of material in a smaller area. Attempts have been made to calculate such shortening and thickening on the basis of folds. Three factors, however, upset these determinations: faulting and thrusting, cleavage, and rock flowage. Reverse faults and thrusts tend to increase the factor for lateral shortening, normal faults tend to decrease it. Flowage is probably the most evasive factor since little is known about rock flowage. Cleavage is closely related to rock flowage (flow cleavage), and whenever prominent cleavage

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appears, an appreciable amount of flowage must be taken into consideration.

New methods have been introduced in recent years in an attempt to analyze deformation and its directions in all its stages. Structures and mineral orientations are measured statistically and in great detail. However, the results obtained are, as a rule, not quantitative.

Quantitative measurements are possible, however, if the original shapes of deformed units are known. Observation of distorted fossils, elongated pebbles or amygdules and other forms have been recorded, but systematic measurements have rarely been undertaken. The purpose of the present paper is to introduce a method which may contribute to the systematic and quantitative analysis of rock flowage in relation to the formation of flow cleavage in non-crystalline rocks of certain portions of the Appalachians.

OOLITES

Oolites are abundant in many sedimentary formations, and in the Appalachian area they are particularly abundant in the Cambro-Ordovician limestones. As a rule, the oolites are originally spherical bodies which are evenly distributed in definite beds and can be traced laterally for many miles. The oolitic beds are folded and many of the individual oolites show deformation into ellipsoids. The deformation of oolites is rarely mentioned in the literature and the author has found no mention of the systematic analysis of such deformation. The wide distribution of oolitic rocks and the possibility of accurate measurements of the deformation of the individual spheres seem to render oolites ideally suited for a systematic statistical analysis of the degree and direction of deformation.

A systematic study was begun in a selected area west of South Mountain in Washington County, Maryland. The results so far obtained justify a preliminary communication. Measurements should be obtained from many localities and the area is too large to be covered by one investigator.

METHOD

A great deal of time can be saved if the area to be investigated is mapped geologically.

Measurements of the amount and directions of deformation are essential. Since spheres are deformed into tri-axial ellipsoids, three axes should be measured. Two thin sections at right angles to each other and oriented in space serve this purpose. Careful orientation is essential and determines the value of all later measurements. In both sections the axes of the ellipses are measured with a micrometer. From 80 to 100 measurements are necessary in order to obtain average values and eliminate originally ellipsoidal forms. In this way the ratios of the axes of deformation and their orientations can be ascertained at any locality where oolites are found. These values can be plotted on maps and sections and if a sufficient number of ratios are measured at many localities, lines of equal ratios may be constructed.

In addition, other observations should be recorded. The oolites vary in composition, size, and shape. Some are surrounded by concentric shells, others contain cores of dolomite, calcite, quartz, feldspar, fossils and other particles. Shells and cores distinctly influence the deformation, and may prevent intense elongation. Many of the measured axes may, therefore, represent minima and not indicate the total amount of extension.

All visible structures in the immediate vicinity of the specimen should be carefully recorded. Bedding, cleavages, lineations, fractures, and fold axes should be measured and plotted with the oolite axes. These will show local deviations in contrast to regional directions of deformation.

TENTATIVE RESULTS

In the following, the results of a reconnaissance study are presented. Seventy thin sections were made from 35 localities in Washington County, Maryland, and in the Chambersburg quadrangle, Pennsylvania. All the oolites were collected from Cambro-Ordovician limestones.

Hagerstown Valley is in a limestone belt which reaches from the Susquehanna River through Maryland into West Virginia and far to the south. East of this belt are the metamorphosed volcanic rocks of South Mountain. From here westward one passes from basal Cambrian upward into the Ordovician.

A strong cleavage, strikes parallel to the fold axes (NNE) and dips consistently east between 20 and 50 degrees. A strong lineation is visible within the cleavage planes of all rocks, including the volcanics. Lineation and cleavage are most intense in the volcanics, and fade out gradually westward and disappear in the limestones near Hagerstown. South Mountain uplift is a large overturned fold and its axial plane cleavage dips gently eastward. Consequently, many of the formations along the west flank of the uplift are overturned, but in normal sequence.

Systematic measurements of oolite deformation were undertaken in several sections across the regional strike.

The ratio: $\frac{\text{long}}{\text{short}}$ axes was measured in thin sections containing the longest and shortest axes. Those ratios are from west to east (Hagerstown to South Mountain): 1.42, 1.45, 2.10, 4.20, 4.45, 5.60, 7.00. The distance between the first and last values is six miles.

The measured ratios were recalculated in terms of extension of the sphere in relation to a unit sphere. These values were then expressed as per cent of extension and the sections showed 18, 20, 44, 105, 115, 138, and 165 per cent. The direction of the maximum elongation is in all cases in the steepest dip of the cleavage plane.

The data gathered so far show clearly that the intensity of deformation increases gradually from Hagerstown toward the summit of South Mountain. There are no abrupt changes, but the area west of the South Mountain uplift has been affected as a whole. The gradual increase of ratios and the normal sequence observed in the field seem to exclude the possibility of a far-reaching overthrust along the western slope of South Mountain.

Folding in this area is accompanied by flowage up to 165 percent and in the direction parallel to the dip of cleavage planes. In the volcanic rocks the ratio is higher if amygdules and sericite and chlorite blebs are measured. In the dolomites adjacent to the uplift, the ratio is so high ($> 7 : 1$) that oolites are largely obliterated. If the ratio exceeds 7 : 1, oolitic bodies are elongated too far for accurate measurements. In view of this high degree of deformation, it seems possible that the highly deformed limestones and dolomites may have been oolitic originally and also may have contained fossils.

Many of the beds in which high ratios were measured are vertical or dip steeply. The longest axes of the ellipsoids, however, are almost perpendicular to the bedding plane (70–90°) and transect these at a high angle. It is clear that flowage has increased the thickness of beds and possibly of whole formations. Measurements of stratigraphic thicknesses in such areas may be subject to revision, and local variations of thicknesses may be due to deformation and not to sedimentation.

Cleavage (flow cleavage) was found to be in the plane which is determined by the longest and mean axis of the deformed oolites. In the area adjacent to the South Mountain uplift, the longest oolite axis is in the steepest dip of the cleavage. West of Hagerstown, it was found to be parallel to the strike of the cleavage planes. Upward from a ratio of 1.3 : 1 cleavage appears and becomes more conspicuous with increasing ratio. The author is fully convinced that cleavage is parallel to the direction of maximum rearrangement of particles due to flowage. The actual extension of particles necessary for the development of cleavage is only approximately 20 per cent. The author is fully aware of the fact that these values are tentative and that generalizations will have to await a more detailed investigation.

Outside of the area in which the influence of the South Mountain uplift is observed, oolites are also deformed but their ratios do not exceed 1.2 or 1.25. Accordingly, all orientations are related to local structures like anticlines or synclines. A general eastward dip of cleavage and maximum oolite axes was not observed. Both are in the axial plane which may be vertical or inclined in conformity with the surrounding major structure.

The Appalachians form an arcuate structure with Baltimore approximately at its center of curvature. Along the inner side of this arc the major oolite axes are radiating outward and converging inward. Proceeding outward (westward) a zone appears where the ratios are low (1.18–1.25) and regional arrangements are not felt. Still farther outward the maximum elongation is parallel to the subhorizontal fold axes, and the ratio increases again (2.00 near Hearthstone Mtn., south of Mercersburg). It seems as if the arcuation had produced a measurable amount of elongation in the direction of the fold axes. If measured on a geologic map, this elongation is about 10–15%, a value of the same order of magnitude as that measured in the deformed oolites.

Lateral shortening of a series of strata by folding is necessarily accompanied by thickening of the entire complex. If the folded complex is 25,000 feet thick and shortening amounts to 50%, the total thickness would become 50,000 feet. Arcuation parallel to fold axes tends to reduce this value. Flowage parallel to cleavage planes may reduce this thickness still more. One hundred per cent extension, as measured near the South Mountain uplift, tends to reduce the thickness by the same amount since it may safely be assumed that the volume of rock has not changed appreciably.

From observations of flowage in oolitic limestones, the author concludes that flowage is a much more important factor than has heretofore been taken into consideration. The limestones west of South Mountain were flattened considerably during folding, and it may be that in an analysis of Appalachian folding this flowage factor has to be considered as most important.

Since oolitic formations are abundant in the Appalachian region, it should be possible to determine the directions of deformation and the amount of rock flowage at many localities with considerable accuracy.

SECTION OF BIOLOGY

APRIL 14, 1941

PROFESSOR RUDOLF T. KEMPTON, Vassar College; *The Problem of Renal Secretion.* (This lecture was illustrated by lantern slides.)

It has been demonstrated by Richards and his co-workers that the first step in urine formation is the ultrafiltration of blood through the walls of the glomerular capillaries. The protein-free filtrate, during its passage along the tubule, becomes highly modified to form the urine characteristic of the species. Perhaps the main process in this change is reabsorption, the return of materials from the lumen of the tubule to the blood in the capillaries surrounding the tubules. Richards and his co-workers have further shown that sugar is reabsorbed from the proximal segment of the amphibian tubule, and chloride from the distal; while water reabsorption is performed mainly by the distal tubule. Probably the reabsorption is conducted in two ways. The removal of sugar or chloride from the filtrate in the amphibia involves cell work. Reabsorption of water from the mammalian tubule similarly requires cellular energy. If, however, substances remain in the tubule during water reabsorption, their concentration may be raised to the point at which outward diffusion into the blood can be prevented no longer. Thus there is a form of reabsorption in which direct cell activity is not involved, the substance merely diffusing outward into the blood.

A second process involved in the elaboration of urine may be that of secretion, the passage of materials from the peritubular capillaries through the walls of the tubule into the lumen. As shown by Smith, Marshall and others, some species of teleostomous marine fish are without glomeruli, the entire renal elimination being secretory. However, glomeruli are usually present in kidneys, and in this case secretion is a process added to the glomerular elimination. Strangely, more foreign synthetic compounds are secreted than normal urinary constituents. However, there is reason to believe that the process is not restricted to foreign materials.

Secretion can be visualized best in tissue cultures of fragments of kidney tubules. In such preparations, the torn ends of the tubules become healed quickly, forming closed segments. If phenol red, an indicator dye, is then added to the medium, the dye enters the proximal tubule cells, passes into the lumen, and there becomes highly concentrated. The dye is red in the alkaline medium, yellow in the slightly acid cells, and red in the lumen. The substance thus passes from a low concentration in the medium to a high concentration in the lumen. This is, of course, in a direction opposite to that expected on the basis of simple diffusion. Interference with the metabolism of the cells prevents this concentration; or, if the concentration had previously occurred, it cannot be maintained and the dye diffuses back into the medium. The mechanism which produces such secretion still remains obscure.

Neutral red is another substance secreted by the intact kidney. Richards and Reisinger have shown that, in the frog, it is eliminated mainly by secretion rather than by filtration at the glomerulus. It is a well known characteristic of neutral red that it follows a pH gradient in either an artificial system or in living cells in general, passing from an alkaline medium to a more acid medium. Evidence has been obtained by Chambers and Kempton which indicates a similar behavior in the intact kidney. The dye appears to diffuse from the alkaline blood or perfusion fluid into the slightly acid cells and thence into the more acid lumen. Upsetting of this pH gradient by the alkalinization of the urine results in a cessation of concentration of neutral red in the lumen; while an increase in the gradient, by increasing the acidification of the urine, results in increased dye output. This elimination continues even though the cells are narcotized, providing the pH gradient is artificially maintained. The secretion of neutral red is therefore a physical diffusion without the direct intervention of cell activity.

While the cause of secretion of neutral red appears to be clear, its method of elimination is quite different from that of phenol red. The secretion of the latter is independent of pH gradients, and is strictly dependent on the maintenance of cell metabolism. Phenol red secretion appears to be more typical of the problem than neutral red secretion.

Various microscopically visible structures in the tubule cells have at times been considered to be morphological secretory mechanisms. Golgi apparatus, mitochondria, vacuoles and globules have all been considered secretory structures. Evidence from various sources indicates that none of these correlations of structure with secretory function stands close scrutiny. Changes in mitochondria which have been associated with secretion of neutral red, for example, appear to be merely the result of the presence of neutral red, perhaps a toxic effect. In addition, the evidence cited above indicates that it is not necessary to hypothecate a structural secretory mechanism for this substance. Vacuoles of concentrated phenol red described in the tubule cells of frogs eliminating this substance are not secretory mechanisms, because, in tissue cultures, secretion of phenol red occurs in the absence of such vacuoles; and, when such vacuoles are present, they do not empty their contents into the lumen. Globules in the proximal tubule cells of Necturus appear to be correlated with the presence of foreign materials in the abdominal cavity instead of being correlated with either secretion or reabsorption. Unpublished studies made by Ruth Castle in our laboratories indicate that there is no direct correlation between secretion and mitochondria or between secretion and the Golgi apparatus. We conclude from the evidence that there is no structure which can be seen under the microscope which is a definite secretory mechanism.

Entrance of secreted substances into the cell and their transport from base to apex of the cell can be separated from the problem of passage into the lumen. The evidence, at the present time, indicates that entrance into the cell and its transport from one end of the cell to the other can be explained on the basis of diffusion, and that the process of secretion is concerned with the passage of the material across the cell membrane which separates the cytoplasm from lumen. In the first place, the apical cell membrane appears to allow passage of dye through it in only one direction. It is obvious that in tissue cultures of secreting tubules the dye is passing from the cell cytoplasm into the lumen through this membrane. On the other hand, if phenol red is injected into the tubule of a kidney with intact circulation, and the tubule blocked to prevent the dye being washed along the tubule by the glomerular filtrate, this dye remains visible over long periods of time. So far as can be determined by microscopic examination, the dye does not enter the cells from the lumen. Even when highly concentrated solutions of dye are injected, the color of the tubule cells remains unchanged. This membrane, therefore, appears to have a one-way permeability, in so far as direct observation can establish this possibility. The basal end of the cell, on the other hand, appears to be freely permeable in both directions to phenol red. This is indicated by two lines of evidence. First, while it is true that in tissue cultures the tubule cell cytoplasm may contain more highly concentrated dye than the medium, this may be due to binding of the dye molecules to the cytoplasmic proteins with resulting immobilization, as far as diffusion is concerned. That this is the explanation, rather than some peculiarity of the basal cell membrane, is indicated by the result of interfering with cell activity. When cultures, in which tubules contain concentrated dye, are treated to inhibit cell activity, the dye diffuses outward. Since this treatment removes the one-way permeability of the apical cell membrane, it is to be expected that any one-way permeability of the basal membrane is suppressed likewise. However, when equilibrium is reached, it is found that the dye concentration in the cell is unchanged. Since, under these new conditions, the dye continues to be more concentrated in the cytoplasm than it is outside, it follows that the relationships across the membrane have remained unchanged and that there must have been a free permeability of this membrane originally. Secondly, the coloration of the cells varies with the concentration of phenol red within them. If small amounts are present, it is all converted to its acid yellow state. With higher concentrations, some of the phenol red retains its red color, while the coloration of the cytoplasm becomes orange. With increasing concentrations, all shades of orange are attained until the yellow is quite submerged and the cell appears red. In a series of cultures with varying concentrations of phenol red, all the proximal tubules of a given culture attain the same color and shade. On the other hand, there is great variation in the different tubule fragments as to the amount of concentration within the lumen. The color of the cells appears to vary consistently with the concentration of dye in the medium, and not at all with the amount of secretion into the lumen. The most logical explanation is that the entrance of dye into the cells is physical diffusion, while its passage into the lumen is correlated with cellular metabolism. We therefore conclude that the basal cell membrane is freely permeable in both directions to phenol red, but the distal membrane allows only one-way passage. Diffusion explains the entrance of the dye into the cell and its movement from base to apex of the cell. Secretion, involving cell work, is the passage of dye across the cell membrane which separates cytoplasm from lumen.

One explanation of this apparent one-way passage across the apical cell membrane is that the configuration of the membrane is such that it permits the passage of dye molecules through it in one direction but not in the other. This configuration might be maintained by cell activity. While this is possible from the viewpoint of colloidal chemistry, there is no direct evidence at the moment which proves that the apical cell membrane is of this nature. Attempts will be made to obtain critical evidence. It seems worth while to point out, however, that if such a membrane were present it would act as a secretory mechanism. When two concentrations of a dye are separated by a freely permeable membrane, the net change is a passage of the dye from the more concentrated solution to the less concentrated until equilibrium is reached. However, dye molecules are actually passing in both directions, and the final result is due to difference in the number of molecules passing each way. If such a membrane were replaced by one allowing passage in only one direction, molecules could continue to pass from the lower concentration to the higher, but not in the opposite direction. The net result would be an apparent reversal of the usual diffusion. If this new membrane were the cell membrane, and, if this characteristic were maintained as a result of cell activity, dye could pass from its lesser concentration in the cell to the higher concentration within the lumen of the tubule. In other words, secretion of phenol red would result from the physical structure of the apical cell membrane.

A complete explanation of secretion must take into consideration the presence of mechanisms for the concentration of materials of recent origin. Smith and Marshall have suggested that this is due to the persistence of a primitive condition now little used for normal urinary constituents. In connection with the secretory mechanism suggested above, it should be pointed out that one of the most characteristic features of tubule cells is that they present a barrier to the outward diffusion of materials from the higher concentration within the lumen to the lower concentration outside. It seems likely that this is a characteristic of the membrane between cell and lumen, for otherwise the cytoplasm would have to be in equilibrium with a very unusual medium, in some cases very hypotonic and in other cases very hypertonic. This barrier to outward diffusion is dependent on cell activity. If this barrier prevents the outward diffusion of phenol red molecules, along with others, while permitting the inward diffusion of phenol red, it could be the mechanism for secretion. Thus, this barrier to outward diffusion, which has evolved through thousands of years, could "inadvertently" secrete a substance synthesized for the first time in the twentieth century.

Whether this suggested secretory mechanism is correct or incorrect, it appears at the present time that it is not necessary to invoke a secretory mechanism to explain the entrance of secreted substances into the cell or their distribution through the cell. It further appears that the Golgi apparatus, mitochondria, vacuoles, globules and other microscopically visible cell components are not responsible for the secretory process. The evidence points toward a secretory mechanism which is much more subtle than this and probably of sub-microscopic dimensions.

SECTION OF PSYCHOLOGY

April 21, 1941

Doctor Frank K. Shuttleworth, College of the City of New York; *Problems and Techniques Involved in the Appraisal of Vocational Opportunities.*

The vocational guidance of high school and college students is one of the important functions of applied psychologists. Good guidance depends on the evaluation of individual aptitudes and the correct appraisal of vocational opportunities. The factor of opportunities is of special interest at the college level because most college students have the abilities, aptitudes and interests appropriate to any one of several fields of work. The Student Personnel Bureau at The City College has recently surveyed its recent graduates in order to ascertain the most promising fields of work. The methods and technical problems involved in conducting such inquiries are of general interest and deserve discussion apart from the particular findings.

On the basis of experience gained in this survey the following suggestions concerning the collection of data may be offered. It is unnecessary to survey all of a series of recent graduating classes. Best results are obtained by surveying college alumni out of school four years. A complete 100% return from one group of 327 alumni suggests that an 80% questionnaire return, exclusive of insufficient addresses, is essential to give representative findings.

In the analysis of the collected data, alumni should be grouped according to undergraduate majors, and each such group should be sorted by educational and occupational status. We have distinguished five categories:—unemployed; in school; employed within the field of undergraduate training; employed outside the field of undergraduate training, but within a field for which a college education is essential; and employed at clerical or unskilled work. With these classifications available, the first step is to appraise past opportunities as indicated by the proportions employed within the field of their training; by median annual earnings; by the extent to which a given field of work attracts alumni from unrelated major fields of study; and by the rewards that different types of employment pay to alumni having the same training. The bare statistical findings on each of these four criteria of past opportunities must be corrected for many complicating factors. For example, the proportion of chemistry majors employed as chemists is low because many are still in school. Again, City College alumni who are practicing law report very low earnings, because all have been employed less than a year. The exact weight to be given to these four criteria is a matter of judgment. Having evaluated past opportunities, the next step is to evaluate opportunities for the immediate future. Four major changes in the outlook must be noted. First, there are the uncertainties introduced by the war; second, teaching opportunities in the academic field in New York City are virtually closed; third, opportunities for the City College graduate in the study of medicine have continued to contract; fourth, the number of undergraduates preparing for social work has increased greatly, thus increasing competition for the available jobs. The uncertainties introduced by the war are wholly unpredictable, but the influence of the other three changes may be estimated with some precision. Were it not for the uncertainties of the war, it is believed that past opportunities corrected for these changes would yield surprisingly accurate predictions of the findings of another survey conducted in the immediate future.

ASTRONOMICAL CONFERENCE

May 2 and 3, 1941

Conference on "The Fundamental Properties of the Galactic System."

Doctor Harlow Shapley, Director, Harvard Observatory, was in charge of this meeting as Conference Chairman.

The program consisted of the following papers:

"The Reference System of Stellar Positions," by H. R. Morgan.

"The Reference System With a View to Planetary Dynamics," by Dirk Brouwer.

"Mean Parallactic Motions," by Peter van de Kamp.

"Mean Parallaxes from Tau-components," by Emma T. R. Williams.

"The Luminosity Function," by W. J. Luyten.

"The Luminosity Function and the Absorption in the Galactic Plane," by Jan Schilt.

"The Magnitude Scale," by Frederick H. Seares.

"The Stellar Distribution in Intermediate Latitudes," by B. J. Bok and D. A. MacRae.

"On the Dimensions of the Galaxy," by Harlow Shapley.

The papers presented at this conference will be published as one article in the Annals of the Academy at a later date.

NEW MEMBERS

ELECTED APRIL 7, 1941

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY

MAY 5, 1941

DOCTOR JOHN B. LUCKE. University of Connecticut, Storrs, Connecticut; Gravel Indications of New Jersey Drainage. (This lecture was illustrated by lantern slides.)

The Fall Zone area of New Jersey in the Raritan Valley near New Brunswick has acquired increasing geomorphic importance as a result of recent efforts to decipher river history on the eastern Appalachian slope.

In his *Stream Sculpture on the Atlantic Slope*, Douglas Johnson proposed that the Hudson River once crossed the Palisades at Sparkill, cut paired gaps across the Watchung Mountains at Paterson and Summit, and after joining the Raritan near Bound Brook, flowed directly to the sea as a master "Hudson-Raritan" in the approximate present position of the lower Raritan River. More recently, in a study of the *Origin and Structure of Pensauken Gravel*, Campbell and Bascom proposed that both the Hudson and Delaware Rivers once crossed what are now the high ridges of New Jersey at the same time, to join in a broad belt of aggradation near Bound Brook, and thence to flow southwestward to Trenton, crossing the Fall Zone approximately at Rocky Hill.

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The present study is based upon a careful search for distinctively fluvial gravels in the Raritan valley. It was found that at least six exposures revealed beds of very coarse, well-rounded stream gravel, differing markedly from the typical Pensauken formation with which they have hitherto been mapped. All the observed outcrops occur upon or immediately east of the Fall Zone. The gravels are regarded as definitely fluvial and essentially *in situ.* They contain boulders of maximum diameters as large as twenty inches, all of which are extremely well-rounded, and in such concentration that pebble counts, comprising a minimum of 100 pieces, were readily made.

The pebble counts indicate complete disparity between the lithology of the observed gravels and the composition of gravels assumed to have been deposited by the present Raritan River. In order of abundance, the types of rock comprising the described gravels are quartzite, vein quartz, vitreous red sandstone, and vitreous conglomerate. These four rock types found in the typically fluvial deposits are present in average percentages of 72, 15, 7, and 3, respectively. All other possible rock types comprise less than 3 percent of any of the distinctively fluvial gravels.

The dissimilarity between the observed deposits and the rocks found in the Raritan watershed indicates deposition by a larger stream draining a region outside New Jersey. The described boulders occur in two local areas in such topographic position that they appear to represent old, high, 110-foot and 70-foot floodplain remnants. In other exposures, boulders of the same character appear to be incorporated as a basal conglomerate in the Pensauken formation. The writer believes, therefore, that they are distinctly older than the Pensauken, perhaps even of Tertiary origin, and that where they are found associated with the Pensauken they have been inherited from the high, floodplain remnants described.

Neither the "Hudson-Raritan" hypothesis of Johnson, nor the "Hudson-Delaware" hypothesis of Campbell and Bascom seem entirely adequate to account for the observed gravels. The field data offer some serious objections to the latter hypothesis and can be regarded as in accord with Johnson's "Hudson-Raritan" hypothesis only by redefining "Raritan." The writer proposes that an ancient Delaware (or any stream regardless of name which drained the quartzite-rich regions of northeastern and eastern Pennsylvania) once crossed the present highland ridges of New Jersey, proceeding directly to the sea in a normal superposed-consequent manner via the present lower course of the Raritan River. The new hypothesis specifically accounts for the high-level gravels described near New Brunswick. It presents, however, no objections to the possibility that both the Hudson and the "Delaware," after merging somewhere near Bound Brook, flowed as one stream to the sea, after Johnson. But since the postulated master stream is enormously longer than any suggested by the name "Raritan," the writer proposes that the tentative conclusions here offered be designated the "Delaware-Raritan" hypothesis, unmistakably drawing attention to the necessity of headwater erosion in Pennsylvania.

The hypothesis postulated as in closest accord with the facts, is that a large, superposed-consequent river, draining an area northwest or west of the New Jersey highlands and here called the "Delaware-Raritan," flowed southeast across northern New Jersey in pre-Pensauken time, possibly being swelled by a junction with an early Hudson River near Bound Brook, and finally crossed the Fall Zone in the approximate position of the present Raritan River.

SECTION OF BIOLOGY

MAY 12, 1941

DOCTOR L. S. STONE, Bronson Professor of Comparative Anatomy, Yale University School of Medicine, New Haven, Connecticut: *Transplantation of the Vertebrate Eye and Return of Vision.* (This lecture was illustrated by colored motion pictures and lantern slides.)

During the past fifteen years, I have been interested in experimental studies involving development and regeneration of the vertebrate eye in all ages from the embryo to the adult. In these investigations, most of the studies have centered about the amphibia, although fishes and mammals have also been included. These researches involve many hundreds of experiments concerned chiefly with transplantations of the entire eye or parts of the eye. For the present discussion we shall confine our attention to the results obtained when the entire functional eye is grafted, i. e., when the eye is excised and either reimplanted or transplanted to another animal. In any case, the eye is normally oriented in the orbit.

When the entire eye is transplanted in fishes, for example, the common *Fundulus*, the graft heals readily and circulation is returned in a few days. For a few weeks the living eye appears normal, then pigment invades the graft. The eye gradually becomes opaque and is slowly resorbed, for it has no power of regeneration.

In various species of frog tadpoles, the transplanted eyes heal rapidly and grow with all the appearance of a normal eye even long after the host has changed into an adult frog. With the exception of a permanent loss of a few ganglion cells in the retina, all the tissues survive. The eyes, however, do not possess the power to regenerate a new optic nerve and, therefore, vision is never recovered in the graft.

In various types of salamanders, the eyes can be successfully transplanted with complete regeneration of the optic nerve and return of visual function. In young larval stages of *Amblystoma*, for example, the eyes usually show little change following operation. There is often a slight temporary decrease in size due to a loss of varying amounts of fluid and blood cells at the time of operation. Circulation returns within a few days and the graft soon reaches the size and appearance of the normal control. This is maintained as long as a year after metamorphosis. Occasionally after operation there is a temporary opacity of the cornea but, if it occurs, it disappears within a few days. The only permanent effect of the operation is registered in the ganglion cell-layer of the retina. This usually involves the loss of only a few cells which takes place during the first few days after operation. The new optic nerve is fully regenerated and connects with the brain as early as 3 weeks after operation. We have shown, furthermore, that the larval eyes between two entirely different species (*Amblystoma punctatum* and *A. tigrinum*) can be successfully transplanted with return of function.

In adult forms of different species of salamanders, the functional eye can also be successfully transplanted with return of all the functions in the graft. This can be accomplished repeatedly in the same eye transplanted several times in succession to different hosts. The common vermilion spotted newt (*Triturus viridescens*) will be taken as an example. The demonstration given in the colored motion pictures shows the technique of operation used on all of the aquatic forms which we have investigated. The animals are anaesthetized in a dilute solution of chloretone. Two animals, in which the right eyes are to be exchanged, are laid side by side on moist cotton. The operation is done under a compound dissecting microscope with needle-pointed iridectomy scissors and forceps. In each eye, the conjunctival sac is first excised, then all the deeper structures including the optic nerve are severed. The eyes are then carefully removed and exchanged between the two hosts. In all cases, the eyes are carefully placed in the new orbits and normally orientated. The adhesive quality of the tissues in the eye socket are sufficient to hold the graft in place so that no sutures are required. The eyes heal in place in a few days and circulation is soon reestablished.

In the living *Triturus* eye, for example, one can see temporary changes in the pigment pattern of the iris. The normal appearance, however, returns in two or three months. Histological sections show that the retina degenerates during the first three weeks, except at the periphery or ciliary margin. From the latter, a new retina regenerates and is fully differentiated by the end of two months. During this time, a new optic nerve is formed connecting the eye with the brain. From two and one half to three months after operation, return of vision can be demonstrated. This is shown in the motion pictures where the animal is prepared for visual tests by first excising the left normal eye. The host then possesses only one eye, the transplanted one. The animal is placed in a small glass aquarium, and when a dark colored lure, such as a piece of wood or rubber, impaled on the end of a wire, is moved about two inches from the outside of the aquarium, the animal swims vigorously about following and snapping at the moving object. The experiment is carefully controlled to eliminate extraneous disturbances such as gustatory, olfactory, and mechanical stimuli. The reactions are the same as in the case of visual responses in a normal control animal.

Regarding experiments on mammals, it may be said, at this point that the eyes of rats at different ages after birth have also been grafted. In no case did the eye persist longer than four months. There was final resorption as in the case of fish eyes. However, at the present time, the opossum is being studied as the most promising type of mammal for eye transplantations. At birth, the young are quite embryonic. The eye, at this time, is in the same stage of development as a five week old human embryo. This condition lends itself to an experimental analysis in a way that is not possible in any other mammal.

In connection with studies of the transplanted eye in different amphibians, interesting observations were made on the ability of the lens to resist degeneration. An exception to the rule is found in the common newt (*Triturus viridescens*), where the lens degenerates completely during the first month after operation. Replacement of the lens, however, took place by a growth of cells from the pupillary margin of the dorsal rim of the iris, as has been shown in cases following lensectomy. From our experimental studies and from the results of a critical analysis of the literature, we have concluded that, with the exception of the small group of salamanders known as the *Triturus*, there is no proof at present that any other vertebrate eye can restore the lens after it is once lost.

We have investigated this problem extensively in an attempt to discover the factors which release this phenomenal growth of the new lens. Various combinations of experimental conditions have been examined in which the dorsal rim of the iris has been transplanted into the eye and the body cavity. The results so far have shown that the release of lens regeneration from the transplanted iris takes place in any of the environmental conditions, but that the presence of a normal fully developed lens inhibits its growth, perhaps through some biochemical medium.

This remarkable method of lens replacement is also unique when one considers the origin and normal course of lens development. In our own studies on lens induction and in experimentally produced cyclopean monsters, we have shown, as well as other investigators, that, through the prechordal mesoderm, influences are released to localize the eye-forming centers in the primitive nervous system. In some way, the eye-forming centers, in turn, bestow upon the adjacent surface ectodermal cells the capacity to form a lens. This means that the original lens does not arise from cells of the eye-forming center which later form the iris. However, it might be possible that the presumptive iris-forming region of the early eye center is the part that is playing the most important role in transferring to the surface ectoderm the capacity for lens formation. In any case, by some unusual event in *Triturus*, this region, now localized in epithelial cells of the dorsal rim of the iris, strongly retains lens-forming capacity. From our own experiments, we know there is no evidence that the iris can release this influence for lens formation to other cells in the way the embryonic eye center does to the surface ectoderm. Therefore, it appears that the epithelial cells of the iris concerned with lens formation can only respond under the proper conditions (loss of original lens) by proliferation. Herein may lie a clue to a better understanding of the fundamental factors which control the general process that we call regeneration.

SECTION OF PSYCHOLOGY

MAY 19, 1941

DOCTOR JOHN VOLKMANN, Columbia University: *Quantum Theory in Psychology.*

Dr. S. S. Stevens and Dr. C. T. Morgan of Harvard University, together with the author, have been interested in verifying and developing a quantum theory of discrimination, proposed in its essentials by G. von Békésy.

Like any other science, psychology should provide fundamental data which can then be used in the solution of specific problems. Measures of discrimination constitute fundamental data. The only difficulty is that the classical methods of measuring discrimination contain certain arbitrary features that prevent the results from being interpreted in a straightforward way. Classically, the difference-limen is defined as the stimulus-increment that is noticed 50 per cent of the time. This definition is arbitrary. Its criterion might just as well be set at 75 per cent, —a figure that some have preferred.

The quantal theory may make it possible to secure fundamental data and to avoid any arbitrary features of method. It assumes, in the first place, that a given stimulus-magnitude produces an all-or-none excitation of a certain number of "neural units" (quanta), and that there is left a small *surplus of stimulation,* less than the amount needed to excite one additional unit. The theory assumes, in the second place, that both the number of units excited and the amount of surplus stimulation fluctuate during the presentation of a non-varying stimulus. The fluctuation may proceed from any source. It may be random or rhythmic. The essential point is that it produces varying amounts of surplus stimulation, each of these amounts being as likely to occur as any other amount.

On the basis of these assumptions, the quantal theory is able to explain in a general way the results usually obtained in discrimination experiments—the sigmoid form, for example, of the psychometric function, which relates the frequency with which a stimulus-increment is noticed to the size of that increment. What is more striking, the theory predicts an entirely different psychometric function in a special case, namely, when the stimulus-increment is inserted very rapidly. There is then not enough time between the standard stimulus and the inserted increment for fluctuation in sensitivity to occur. There can be found a stimulus-increment that will *just* suffice *always* to excite an additional neural unit. It supplements the surplus of stimulation, no matter how small this surplus may be. In the simplest case, in which the excitation of one additional neural unit gives rise to a perception of difference, the size of this increment is the measure of the size of the quantum. This measure is derived from the assumptions listed above. There is nothing arbitrary about it.

Under this special condition of rapid change, then, a certain increment (the quantal increment) will give rise to a judgment of "different," 100 per cent of the time. Correspondingly, a zero increment *never* excites an additional neural unit, and it yields a judgment of "different," 0 per cent of the time. In this way, the theory predicts two points on the psychometric function—at 0 and 100 per cent. Between successive insertions of the stimulus-increment, there is ample time for the sensitivity of the system to change, so that, as previously assumed, an unvarying stimulus will excite varying numbers of neural units with varying amounts of surplus stimulation. The surplus will vary between the limits, zero and one quantum, and its various values will occur with approximately equal frequency. Now a small stimulus-increment will supplement an existing surplus, and excite an additional unit, only if this surplus is a relatively large one. This will happen a small proportion of the time. A large stimulus-increment will supplement either a small or a large surplus of stimulation. Hence it will excite an additional neural unit a large proportion of the time. The more rigid mathematical argument shows that, between the limits 0 and 100 per cent, the frequency with which an additional unit is excited is directly proportional to the size of the stimulus-increment. Instead of being sigmoid, the psychometric function is an oblique straight line.

Von Békésy supported his theory with data upon the discrimination of auditory loudness. He presented tones lasting 0.3 seconds, and, in the middle of this brief time, changed the intensity of the tones rapidly. A few highly practised observers were able to perceive regularly increments which excited a single additional neural unit (first-quantum discrimination). The psychometric functions were acceptably rectilinear. With less practice, the observers required increments which excited *two* additional neural units (second-quantum discrimination). As predicted from the theory and actually obtained, the function was still rectilinear between 0 and 100 per cent, but it was displaced the width of one quantum and had the expected slope. For the theory allows us to predict the slope of this second-quantum function—the slope is such that the increment perceived 100 per cent of the time is twice the largest increment which is never perceived.

Our own experiment upon loudness-discrimination employed a continuous 1000-cycle tone, which, once every 3.0 seconds, was augmented in intensity for 0.15 seconds. A system of electrical filters prevented the transition from one level of intensity to another from being too abrupt. The size of the increment was varied in accordance with a modified form of the constant method. The observer pressed a key, whenever he heard a pulse in loudness, and refrained from pressing, when he did not hear one. Proceeding in this way, he produced a number of psychometric functions, which showed plainly the characteristics of second-quantum discrimination. The slope of the straight lines followed the rule mentioned above, and this fact alone constitutes strong support for a quantal theory.

Although it was proposed by von Békésy specifically for the discrimination of auditory intensity, the quantal theory contains no assumption which limits its scope in any obvious way. It may conceivably apply to the discrimination of many aspects, or all aspects. Realizing this, we examined another case—the discrimination of tonal pitch. Increments of frequency were added, briefly and periodically, to a steady tone, and the observer pressed a key each time he heard one of these increments. The same system of filters prevented the changes in frequency from being too abrupt. Six observers gave results typical of second-quantum discrimination, including the crucial relations of slope. By statistical test, the rectilinear quantal functions fit the data considerably better than the sigmoid functions of the classical theory. There are quanta of pitch-discrimination, then, as well as quanta of loudness-discrimination.

Are the pitch-quanta peripheral or central in origin? To shed light on this question, data from binaural stimulation were compared with data from monaural stimulation. All four observers showed quantal discrimination of binaural stimulation. Three of the four had binaural quanta that were about two-thirds as large as their monaural quanta. The two ears worked together to produce a finer quantal discrimination. Hence, the quantum appears to be central rather than peripheral. However, since we do not know of a correlation with any identifiable structure, the quantum is best regarded as a functional unit.

The size of the quantum varies considerably between observers. Occasionally and for no known reason, it also varies in the results of single observers from one experimental session to another. There is a regular variation of quantum-size with sensation-level, similar in form to the relation obtained earlier by Shower and Biddulph. In absolute value, the quantal measures of differential sensitivity are smaller than the measures yielded by the usual methods.

Following the idea that the quantal theory holds generally, the author has been trying to find a quantum of visual spatial discrimination. The observer looks with one eye directly at a very small, brightly illuminated point. Periodically and rapidly, this point is moved through a small known distance and moved back again. By pressing a key, the observer reports the instances in which he sees the point move. Apparently, the conditions for quantal discrimination are very critical, but as these conditions are more and more closely approximated, the data fall more and more plainly in the curve that is characteristic of first-quantum discrimination. It is a fair conjecture that, in this case, the quanta are peripheral rather than central in origin, and that the variation in sensitivity proceeds from small changes in the locus of stimulation on the retina.

The quantal theory presents a new list of problems for experimental psychology to solve. For example, neither the necessary conditions nor the optimal conditions for obtaining quantal discrimination are completely known. The physiological correlates of quantal discrimination are also not known. If the quantal theory holds generally and if its conditions can be met, it should be possible to find many other specific instances of quantal discrimination. Perhaps there are quanta of psychological magnitude as well as quanta of differential sensitivity. But the most important task of the quantum research is to provide a set of fundamental data in the field of discrimination.

ANALYSIS OF SEA WATER IN A CLOSED AQUARIUM[1]

By

Louise P. Wilson and Mary A. Griggs

Wellesley College

Wellesley College has maintained in its aquarium room, a closed system of circulating sea water which involves the typical features of such systems, that is, a series of aquaria which drain through a large gravel-filled filter into an open storage reservoir from which water is pumped again into the aquaria. The present aquarium water was transported in water trucks from South Boston Harbor in December 1933. The system holds 14,000 gallons to which approximately 100 gallons of lake water are added weekly to correct for evaporation. Sodium bicarbonate is added periodically in order to maintain a constant hydrogen ion concentration. An average of 12 pounds a year has been used since 1934. Less is required immediately after cleaning the filter and progressively more as the filter becomes clogged with organic wastes. Each of the three large display aquaria are constantly aerated with compressed air. No temperature control is attempted.

To maintain animals in a healthy condition for exhibition and experimental or life history studies, it is essential that the chemical and physical properties be kept as nearly like those of normal sea water as possible. In order to test the success of this laboratory in maintaining such a suitable salt water environment, examination of physical properties and spectrographic analyses of certain mineral constituents of the aquarium water were made and compared with like analyses of water from Boston Harbor.

Analyses were made on four samples of sea water collected under the following conditions:

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[1] Special Contribution to the Transactions. This paper was not presented at an Academy meeting.

I. Boston City Aquarium. Collected November 1939. An unknown volume of fresh sea water had recently been added to the water which had been in the aquarium for a year.

II. South Boston Harbor. Collected November 1939, high tide.

III. Wellesley College Zoology Aquarium. Collected November 18, 1939—twenty four hours after the supply tank had been filled with fresh lake water.

IV. Wellesley College Zoology Aquarium. Collected November 9, 1939—six days after the supply tank had been filled with fresh lake water.

For comparison, the following table also contains results obtained by using analytical chemical methods:

V. Composition of Woods Hole sea water (1).

VI. Probable composition of sea water as computed from various sources by Robertson and Webb (2).

The only preparation required for the analysis was quantitative dilution for some of the determinations. Calcium, magnesium, potassium and sodium were determined by the Lundegardh spectrographic method, the intensity of the lines being read by means of a Zeiss Spectrum Line Photometer. The chloride was determined by titration with silver nitrate. The results are as follows all expressed in grams of the metal per liter of sea water:

Sample	Calcium	Magnesium	Potassium	Sodium	Chlorine	pH (3)
I	0.39	1.2	0.49	9.0	18.2	7.67
II	0.39	1.2	0.47	9.4	17.8	7.65
III	0.40	1.2	0.57	10.8	18.4	8.22
IV	0.42	1.3	0.60	11.0	18.9	8.10
V	0.428	1.300	0.412	8.8	18.35	8.3
VI	0.42	1.31	0.39	10.7	19.3	

The aquarium waters differed appreciably from the sea waters only in the potassium content. In order to check these potassium results, since there were no records of potassium analyses on the water from South Boston Harbor, water from Woods Hole, Nobska Beach was analyzed. The results in grams of potassium per liter are as follows:

1st portion	0.45
2nd portion	0.44
3rd portion	0.43
4th portion	0.43

Two further tests in which known weights of potassium chloride were used showed an error of only 1% in analyses made by the same method. The result for potassium in Woods Hole water is close to that obtained by Page (1) and agrees with results obtained by Wheeler (4) for Beaufort Harbor water. It therefore seems reasonable to suppose that potassium accumulates in the aquarium from the decomposition of food and waste products. Practically all added food is fish muscle. The same factor probably accounts for the high potassium content of Boston Harbor. Contrary to our results, Breder and Howley (5) show a slight decrease of potassium in the circulating system of the New York Aquarium.

All results for the water from the Wellesley aquarium are slightly higher for the sample collected six days after filling, than for the sample collected only one day after filling. This, of course, is reasonable, since there is constant evaporation which would leave the minerals more concentrated. It is noteworthy that the principal ions of sea water may be kept so nearly constant without elaborate methods of chemical control.

In 1936 and 1937, daily records of specific gravity and temperature were made. The specific gravity, measured with a hydrometer and corrected to 15° C., varied between 1.020 and 1.026 throughout the year. Temperature in the storage tank varied from an average of 20.8° C. in February to an average of 22.5° C. in August. The lake water used for dilution varied from as low as 4° C., in winter, to 19° C. in summer. The cool basement storage tank in summer and the heating plant in winter tend to equalize the temperature.

A brief study of the pH of the water from the aquarium was made in January, 1940, by the use of the Beckman pH meter. Samples were collected on seven consecutive days and the pH was always within one hundredth of a pH from 8.29, even though the fresh water was added after the sample for the second day had been obtained. This indicates the complete buffering of the sea water probably aided by the sodium bicarbonate which may also account for the high alkalinity.

Among the animals kept constantly in the aquaria, are species of bony fishes, Arthropoda, Mollusca, Coelenterata, Echinodermata, and a few Annelida. *Limulus* has moulted normally in the aquarium for four years and *Busycon* has been kept for six years. Tautog and sea bass, among the vertebrates, have been kept five years. An encrusting coelenterate has maintained itself for several years and has spread over the entire aquarium.

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SECTION OF GEOLOGY AND MINERALOGY

OCTOBER 6, 1941

PROFESSORS J. T. STARK, W. E. POWERS, and A. L. HOWLAND, Department of Geology, Northwestern University; PROFESSOR J. HARLAN JOHNSON, Department of Geology, Colorado School of Mines; PROFESSOR C. H. BEHRE, JR., Department of Geology, Columbia University; and PROFESSOR DON B. GOULD, Department of Geology, Colorado College: *The Geologic History of South Park Colorado.* (This lecture was illustrated by lantern slides and was presented by Professor Behre).

South Park is a great depression in central Colorado. Though lying at an altitude of about 9,000 feet, it is almost surrounded by high ranges of the southern Rocky Mountains, some of which rise to altitudes of more than fourteen thousand feet. In sharp contrast with these towering peaks, the Park is characterized, especially, by its remarkably even surface. Its relatively barren floor is marked by several almost flat sub-levels, in places greatly dissected and isolated, but elsewhere rising in successive steps and thus suggesting wave-cut benches, pediments, or stream terraces. Locally, there are also hogback ridges, reflecting monoclinal dips

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of resistant beds. The marginal wall of high mountains is broken only to the south. Here, a low divide of a few hundred feet separates the Park surface from the Arkansas Valley. Despite this breach in the encircling ranges, the two streams draining the Park, the South Platte and the Tarryall rise in the mountains that form the western wall and flow eastward across the Park and across the Front Range; thus suggesting a history of superposition or antecedence.

In brief, the generalized picture conveyed at first glance is one of a fault graben or of an old lake bed, with some complications in structure and in detailed physiographic history. On the floor of the Park and the bordering elevations, numerous erosion levels are believed to have been recognized, but no detailed study has been devoted to the geologic history of the unusual depression represented by the Park itself.

As a result of three seasons of field work, a far more complex development of South Park than that implied above has been reconstructed. The facts, upon which such a history is based, are not only physiographic but structural, stratigraphic, and petrographic as well. It has proved possible to determine the age of structural features of earlier origin in the usual manner, by reference to the stratigraphic succession, which ranges from pre-Cambrian to Pleistocene, the Silurian being the only period unrepresented by sediments. Particularly helpful has been the finding of vertebrate fossils at several horizons. These have enabled a dating of physiographic events far more exactly than hitherto for this region.

Though the earlier history is interesting, it will not be set forth here. The episodes which contributed most significantly to the present form of the Park began with the Laramide revolution. This was initiated by an uplift to the west, leading to erosion of pre-Cambrian and younger rocks and to the deposition, mainly by eastward flowing waters, of at least 7,000 feet of Denver (Paleocene) beds. Later came the Elkhorn fault, first recognized in 1910 by Washburne. This crowded pre-Cambrian crystalline rocks westward over the Denver sands and gravels. The Elkhorn is a low-angle thrust that has been traced for 36 miles along the eastern edge of the Park. It is one of the major displacements of the southern Rocky Mountains. Three other large reverse faults cross the Park in a northwest direction. Their throws are of the order of 500–1000 feet, but they obviously cross the "grain" of the topography and had little to do with the present form of the Park. Despite careful search for larger displacements and despite the presence of many minor normal faults, there is no evidence that the topographic basin of the Park is to be assigned to simple graben faulting. Its present form, however, was certainly outlined during the Laramide revolution by two structural features: (1) the monocline of resistant Paleozoic beds which makes up the eastern slopes of the ranges west of the Park; and (2) the escarpment, now largely a fault line scarp, of the Elkhorn thrust, east of the Park.

Some time after the Laramide revolution, the structural basin was etched out into a broad river valley. The Tertiary history of this basin included lacustrine deposition; vulcanism, especially in the south, yielding tuffs and flows; and some later folding and faulting. The earlier lavas apparently acted as ponding agents, causing lake deposits to form in the Oligocene and early Miocene. The later Tertiary sediments were chiefly fanglomerates or river alluvium, however, and were still being deposited during the Miocene and possibly early Pliocene. These youngest beds dip in the direction of the former southward course of the stream that drained the Park into the Arkansas. The last effects of the South Park river were of Pliocene age, a remarkable strath which truncates the alluvial strata underlying the low, hilly divide that constitutes the southern boundary of the Park. These truncated beds contain vertebrate remains of the early Pliocene or late Miocene. Subsequently, there seems to have been tilting *downward* to the north, which favored the headward capture of the southward Park drainage by streams whose growth was across the Front Range. The reasons for this piracy are still not wholly clear and must await detailed studies farther to the east, for the geology of the Park itself sheds no light upon this question.

After the drainage diversion came some alluviation, probably coupled with glaciation. Two stages of late glaciation are clearly recorded by as many sets of moraines and terraces, and three earlier glacial stages are suggested by a corresponding number of higher alluvial terraces. These glacio-fluvial (?) terraces constitute the five conspicuous sets of flats recognizable within the Park. All are below the level of the divide at its southern margin. Thus, glaciation had no effect on the fundamental problem of the origin of the Park as a whole, but it greatly modified the detailed topography.

In brief, then, the Park was first outlined by the Elkhorn fault on the east and the eastward dipping monocline on the west. The basin, thus formed, was etched out by stream work, was, in part, the site of lakes, and was, variously, modified by vulcanism. Later the main, southward flowing river was beheaded by one or more streams having an eastward gradient. There are no simple erosion levels in the Park at elevations less than 12,000 feet, except that crossing the southern divide. Contrary to statements by earlier investigators, this is younger than the upper Miocene at earliest, since it bevels rocks of that age. The general form of the Park, today, thus bears traces of its original Laramide structure, of its pre-Pliocene stream erosion, of Pliocene tilting and headward trenching by east-flowing streams, and, finally, of its Pleistocene alluviation and glaciation. However simple this history may appear at first sight, it proves to be highly complex when the facts are more carefully examined.

These studies were made possible by a grant from the Penrose fund of the Geological Society of America and by a research grant from Northwestern University. The authors wish to express their appreciation for these financial aids and for permission given by the Geological Society of America to publish this preliminary description of a part of their work.

SECTION OF BIOLOGY

OCTOBER 14, 1941

PROFESSOR OTTO GLASER, Amherst College, Amherst, Mass.: *Pattern and Growth.* (This lecture was illustrated by lantern slides, patterns, stagger-patterns, and models.)

I

What symptoms of pattern can we expect in a growing organism? The answer is not entirely simple. Were we confronted with an isolated and unlabelled egg 100 microns in diameter, we should require the services of a particularly specialized specialist to tell us what sort it might be. After the egg had undergone numerous divisions and the resulting mass had increased sufficiently in size, less specialized specialists would be able to hazard a guess. However, only after further growth and transformation would the general nature of our creature become an open secret in biological circles. All of us would then recognize the basic pattern of a vertebrate. Subsequently, even a layman could suspect a mammal and before long, recognize a dog, and finally, a spaniel.

Were we to adopt this individual, we should discover in due course, a combination of size, weights, proportions, colors, markings, habits and temperament, rare enough to require a special name. This name would not be included in any reliable work on taxonomy, but if our spaniel were sufficiently remarkable his name sooner or later would be inscribed in a stud-book. An important item in the specialized fancier's vocabulary would be our particular spaniel's name.

Although the specificities under consideration complicate our problem, in the long run, they are also the keys to understanding. If we would ask the geneticist just when our spaniel decided to be not like other spaniels, we should receive a clear-cut answer. He was not like other spaniels from the outset. Toward the end of his unicellular career the act of fertilization added to his earlier endowment of maternal genes an equal complement of paternal genes. His maleness is traceable to a particular substitution of genes. These invisible units are protein in nature and within their forty-eight recognizable associations, orderly in array. Given the conditions under which a spaniel occurs at all, the genic endowment of this one, implied all the additional specifications needed to terminate in an individual classifiable by type yet too unique for domestication to go without his special name.

How the genetic proteins determine this end-result is momentarily not certain. Conceivably the genic master-pattern could undergo either extension or, like an ancient text handed down in many languages, appear in some form of free translation. These two suggestions are not alternatives. According to the estimates, the quantity of genetic proteins present at a given moment in the average human adult is sufficient to repeat the original endowment in the fertilized ovum, not less than $2.8 \times (10)^{13}$ times. Could all these repetitions be made contiguous, no doubt something accurately measurable in two dimensions and with special techniques in three, would become visible. Yet, until these or equally significant measurements become available, we can draw no precise picture of the genic pattern. All that we can be certain about, now, is that the organism and the developmental stages we see are not direct geometrical enlargements of that pattern. Whatever it is, it manifests itself during the life history of the individual only as a consecutive series of specific "translations." The members of this series differ in volume and shape, but each is implicit in the other and all, after the manner of stagger-patterns, determined by an original pattern or text. Accordingly, if, even direct topological identification of the genic pattern with its end-results or any of the stages intervening, is excluded, we can expect to find outward and visible signs of an inward pattern only if organisms at all times have properties invariant in all patterns and independent of all direct optical manifestations.

II

Specificities in minutiæ are conveniently explained by Mendelian mechanisms. Specificities in the gross illustrate the Biogenetic Law. Between the two types there is no type-difference. Both involve the genetic constitution and in the absence of growth with its inseparable context of simultaneous specific transformation, neither the small nor the large specificities could be recognized. In this association, items permanently isolated from their context become unintelligible. Yet, the nature of our problem forces us into the realm of abstraction. Ideally, we require a characteristic of growth that is also common to every pattern and at the same time faithful to the true nature of the organism. Here, we are confronted by a system of dilemmas quite hopeless were it not for the metrics of growth.

The attempt to describe every conceivable type of pattern reduces us to the level of a simple slogan: Whenever we have so much of this we also have so much of that. This statement like its formalized version, $y = bx^{\alpha}$ is a brief summary of all the work in allometrics. When x and y represent consistently any two dimensions of the entire organism, of its parts and organs, or of any two native chemical fractions, these quantities, if consistently paired over relatively wide reaches of size or age, exhibit fixed ratios. In the equation $y = bx^{\alpha}$, b represents the value of y when $x = 1$. The constant α asserts that a change in x of 1 percent is associated with a change in y of α percent. This relation is most conveniently stated in logarithmic form: $\log y = \alpha \log x + \log b$. In this form x and y are linear functions, (10: 22).

At the moment, the absolute values of x and y are not especially important. However, the value of α and particularly its behavior, have immediate significance. Alpha reflects not only the interrelations among the variables we have measured, but, also, how these measurements were used. Whether we compare a given kind of y with a certain type of x or do the opposite is unimportant, if we do not intend to make other comparisons. The two resultant α's are both constant and merely repeat so much of this for so much of that, antiphonally. However, when we wish to compare our first α with the α's derived from other sets of x's and y's, it becomes highly confusing if the relations between our first standard and its comparate are stated in one way and those of the second pair in the other. Worse still, the kinetic relations of x and y in one set of pairs can hardly be identical with the same relations in all other sets of pairs. In the literature on allometrics (5) almost everything has been compared with almost everything else, in almost every conceivable way. Yet, despite these ineptitudes the organism continues to assert its coherent nature. A random sample of 275 α-values fluctuates about unity. Twenty-five percent of these differ from unity by 5% or less and in 11% of the cases, $\alpha = 1.00$.

Apparently the organism is not a hiding place for irrelevancies. If this appraisal is correct, items whose relationship to one another in a given organism is immediately clear, should yield α's more highly concentrated about unity and more frequently equal to one. Various dimensions of the growing chick and of its growing heart demonstrate the point (1). It can be demonstrated also over a wide range of organisms if we limit ourselves to significant index substances such as water and the chlorides or phosphorus, creatine, creatinine, and purine. All these, in various combinations, yield α's close to or equal to, unity. These cases are only a partial exhibit of the system of interrelationships named by Needham: "the ground plan of animal growth," (22).

Setting apart all specificities, even those of size and age, critical allometry uncovers among the structures and processes of the organism specific metrical relations. If one of the variables in this system changes by one percent, certain others also change by one per cent. Relations of this type are called isogonous. Were they the only ones, a spherical spaniel's egg could grow only into a large spaniel sphere. Alpha cannot always be unity. On the other hand universal heterogony, even if conceivable, could result only in the unrestrained complexity that is chaos. Every normal growth must have its isogonic core. This includes all the processes and items whose adjustment to one another must remain constant, within exceedingly narrow limits. As the organism successfully domineers over the rigors of development and increasingly struts its dramatic diversifications, the isogonic underpinnings become obscured by their heterogonic canopy. Yet, even within the substance of the canopy itself, rates heterogonic with reference to the core are, or become, isogonic with reference to each other. Thus, all the α values for a given organism ultimately fall within a narrow zone. But, whether the relations they disclose in certain phases are 1 to 1, 2 to 1, or 3 to 1, α, in the end, always tells us how much of this goes with how much of that. Unless we insist on purely optical criteria, these metrical interrelations are more than symptoms of pattern. They are pattern in their own right.

III

Every pattern in a growing organism is at once a symptom and a herald. As a symptom, it becomes inexplicable without precursors, and if not a herald, it spells the cessation of development. If we desire insight into the processes by which precursors translate themselves into their own symptoms, which, in turn, are ultimately recognized as heralds when further specific transformations follow, the processes in question must be studied while they are going on. This calls for accurate timing. If we wish to study the growth of chickens, we cannot call the day of hatching, zero. This practice, still defended in other connections (10) omits the whole of incubation time. However, had the preceding 21 days counted as nothing, the chick would not be available to be misunderstood. Necessarily, when we make our observations, we must know the correct time.

We must also be serious about other things. We must choose periods of growth free from external interferences and protect our measurements against contaminations traceable to novelties internal in origin. We must always measure or weigh the same systems or items and distinguish between a natural fact and an analytical artifact. Success with individual items and constellations of these, depends on the sensitivity of our physical and chemical methods and on how we use them. Constellations that cannot or should not be taken apart can be dealt with as entireties provided their constituents maintain constant proportions and have orders of magnitude large enough to completely swamp any minor contaminations. No one determining the total weight of a growing chick could possibly recognize the fraction due to the copper that is present. On the other hand, were the copper determined as a natural or artificial associate in another metallic isolate, equally rare or rarer, its presence in this complex would make an important and recognizable difference.

Under these critical conditions, adequate temporal analysis of growth in the entire organism, its organs, parts, or native chemical fractions, discloses in all of them increments stamped with the family likeness of compound interest. Yet there are important differences. The compounding occurs not at fixed intervals of time, but continuously. Moreover, the rate of interest falls. All this enables us to formulate a second slogan: More makes more, less and less.

In our second slogan, "less and less," implies an acceleration. Saddled with this, no organism can produce a constant percentage of itself during any two consecutive constant intervals of time. Were we to demand this, the organism could comply only if we were willing to make the second interval longer than the first. However, every type of chronometer we employ has a scale which divides time into a series of arbitrary units numbered consecutively from zero up, and equally spaced. The numerical value of intervals on these scales is the difference between two consecutive whole numbers, and is always unity. When we add all the units between a given point and the zero, the sum equals the total time that has elapsed since it was zero o'clock.

What the organism asks for is a new scale. Some of the advantages of the old can be preserved if the new scale coincides with the old at zero time and has the same number of intervals. Each interval, however, must be longer than its predecessor. If we are to pass freely from one scale to the other, such stretching of the intervals should be systematic and for all intervals proportional to their distance from the zero point. If an interval on the old scale is definable as the difference between two consecutive whole numbers, all requirements will be met by the new chronometer if we define an interval as the difference between some power of consecutive whole numbers. By inspection, the power required is found to be two.

No available chronometer incorporates this principle. However, since we can pass freely from one scale to the other, old-scale readings can be treated as if we had used the new scale. By the old scale, the productivity of the chick goes down as the amount of time that has elapsed since the zero hour goes up. Hence, if the data derived from the chick itself are to remain inviolate we must employ, not the differences between the squares of whole numbers, but the reciprocals of these. The temporal datum we require for each interval is $1/(2t+1)^2-(t)^2=1/2t+1$. With the aid of this expression and a little calculus the relation between time and the growing weight of the chick, w, can be formalized. In logarithmic version it reduces to $\log w = k \log (2t+1) + C$ (5). This is the translation of our second slogan into the language of mathematics. It has the following implications. At a given new-scalar time, $2t+1$ old-scalar units from the zero point, the total weight of the chick is the product of its initial weight, C, compounded continuously at a rate that falls by a constant fraction k, of the intervals between the squares of the time units that have elapsed since growth began.

If our temporal analysis is faithful to the data, it should be possible to calculate not only what a chick of given age may be expected to weigh, but how much of a particular material it is likely to contain. For the chick as an entirety, it is possible to calculate weights from ages with fair accuracy in all cases and with complete satisfaction in some, (cp. Lerner; 12). This is essentially true also of the chlorides. In every organism, these are related by constant proportions and hence can be manipulated as a unit. When they are so treated, it becomes possible to calculate the average amount of Ca taken by the allantois from the shell. The orders of magnitude determined by chemical analysis and the orders based on the equation are identical. Even better agreement is to be found in the case of water. Here, observation and the products of the equation are such that one set of values, plotted against the other, distributes the points on or along a straight line whose slope of 45° demonstrates a 1 to 1 relation between the two sets of values. Better yet, we can relate temporal with allometric analysis. From the latter, we learn that the percentage changes in y are α times the percentual changes in x. In temporal analysis the percentage changes in x and y are respectively k_x and k_y times the percentual changes in $2t+1$. It follows that the ratio $k_x/k_y = \alpha$. Accordingly, every conceivable ratio among the k values, established for the chick and a dozen entities, categories, and processes of the chick, should fluctuate about unity, while ratios based on data having the closest biological connexity, should be unity. Since these expectations materialize (5; 8) we have a bridge from allometry to the metrics of true growth.

IV

What other implications has $2t+1$? While the organism is growing, processes of two types go on at the cellular level. First, the total number of cells increases; and, second, an ever increasing number of these cells undergoes structural differentiations that incapacitate them for further mitoses (25). In the terminal adult, tissues, in which replacements occur, are normally the only sites where cell multiplication can be found. All cells, whether they continue to divide or not, ultimately contain the same genetic proteins in equal quantities. Clearly, however, the rate at which new genetic proteins can be housed in new nuclei declines.

As the most easily available index substance, we select purine nitrogen. According to our analysis, k for purine nitrogen and k for the chick are identical. Alpha, therefore, is unity. This value is exactly what we should expect for it is hard to imagine how a chick could grow normally at any rate other than the one characteristic of its genetic proteins.

We single out these proteins not because they are necessarily different from others, but because many pertinent facts are known about them which are not known about other proteins. We know that they are present in chromosomes whose behavior in the sex cells leads to predictions that are quantitatively verifiable. Long ago (24), geneticists began to map the chromosomes indirectly with the aid of evidence obtained from adult fruit flies. Today, the giant chromosomes in these organisms are photographed. The photographs exhibit cross striations in orderly array. These bands, like spectral lines, are identifiable by their locations and many of them have been numbered. Between the chromosome photographs and the indirect genetic maps, as well as between either of these and the occurrence of specific traits in the adult, 1 to 1 relationships are demonstrable. It is fairly certain that we know where particular genes or gene complexes are located. These maps are patterns, not of the genetic proteins or even genes, but only of their distribution. Are the genetic proteins themselves patterned? This question cannot be answered directly. However, if they are patterned, which is not a new issue (4), and if they are also not categorically different from other proteins, all native proteins wherever or however located should exhibit at least some of the fundamental properties of pattern.

Every pattern is a matter of repetition. It is not for that reason inevitably monotone. It may be self-limiting or limited in space by the conditions under which it occurs. In either case, it can be repeated, extended, or repaired only if the various units whose arrangement compose it are all available in sufficient numbers. When this is true, the pattern itself determines how many examples of a given type of unit can fit into the limited number of specific complementary locations. Once these are occupied, all units that remain unaccommodated become useless with reference to this particular bit of pattern.

All this applies to any pattern, but it is also a generalized blue print for protein metabolism. We can refer to only a few of the elementary data. Growth is checked by the absence of lysine; repair, by the absence of tyrosine. No organism permanently harbors amino-acids except in the specific localities available in its native structural proteins. The units that are not so accommodated are destroyed and furnish the nitrogen in uric acid or urea. No matter how much the nitrogen income may exceed the actual requirements, the organism remains in nitrogen balance.

Most of this knowledge is based on adults. During its growing phases the organism not only maintains what it has grown, but continues to grow. Its nitrogen income, therefore, must be relatively greater than in the stabilized adult. Nevertheless, as Schoenheimer (23) has so beautifully shown, the difference between the protein metabolism of a growing and an adult individual is not one of principle. It depends essentially on a shift in equilibrium. In the young, the sum total of synthetic processes exceeds the destructive. In the adult, synthesis, though very lively, is cancelled by destruction. Even greater significance attaches to Schoenheimer's results with isotopic ammonia. In birds, the nitrogen, so marked, registers in the purines and, also, in the uric acid. The genetic proteins, therefore, can no longer be considered as if they were completely isolated from the general metabolic stream. Like other proteins, they, too, are involved in both synthesis and destruction.

For the growing chick the absolute quantities of purine nitrogen and of uric acid (21; 5) together with their rates of accumulation, are all known. Very little arithmetic is required to show that if all the nitrogen in the uric acid were derived exclusively from the purines, the chick would be either without any genetic proteins or forced to produce double the quantity ever actually present. In this dilemma, our k values become decisive. K for uric acid is of the order 3.96, whereas, for the chick $k = 3.76$, and for purine nitrogen, 3.77. As the total nitrogen bound in uric acid equals the total bound in purine, since k for uric acid is larger than k for purine, and, finally, since the rate of purine synthesis could hardly be doubled without also doubling the growth rate of the chick, only one conclusion can be drawn. The nitrogen in the uric acid must have other sources. It cannot be attributed exclusively to the genetic proteins.

With protein metabolism of the embryo and adult in accord, we can take the next step. In the organism, between the hazard of destruction and the opportunity of incorporation, every population of drifting amino-acid units always disappears. Both influences may be at work simultaneously. Ceaseless deaminization, regardless of amino-acid type, reduces the population by a large fraction without changing (except by chance) the ratios or proportions of the various amino-types. The non-nitrogenous remnants of the disrupted individuals are salvaged, yet, whatever their subsequent forms and fates, these are controlled in the last analysis by associations of units that have not been destroyed. When these are finally "on the track" to survival, their departure from the floating population involves the quintessence of specificity. Only units of given specific types locate in or on certain of the native proteins, provided these have niches specifically complementary to the incoming units. Of all the individuals ever present in the floating populations, the settlers are the only ones that count. It is, also, they that decisively influence our measurements. Even the local specificities of quality or order of magnitude that beset the organism stand in some proportionate relation to the qualities and quantities of specific proteins. Everything the organism is, has, or does, may be traced at long last to its protein machinery. It may work in ways that are dark and peculiar, yet its operations reverberate throughout the entirety (6; 7). If, despite local specificities, this influence is all-pervasive, the only dimension in which the proteins could always leave an easily legible signature is the temporal dimension.

When we determine a growth rate, our balances record, to date, the net total effect on weight, exerted directly and otherwise by all of the protein settlers. Toward the end of incubation, these represent 10 per cent of the chick's total weight and 57 per cent of its dry substance. Our clock-records should tell us not only how much time has elapsed since active protein settlement began, but also how much longer it takes, on the average, to settle today than it did yesterday. Both pieces of information are given simultaneously by our $2t+1$ chronometer.

But $2t+1$ is the difference between the squares of two consecutive whole numbers. Can this have any bearing? Since our measurements can take account only of settlers, the number of these equals the number of specific locations they come to occupy. We do not know what these numbers are, but we do know from elementary statistics what the chances are that a unit of specific type will occupy its complementary specific niche. These chances equal the product of the number of units multiplied by the number of niches. Since these numbers are equal, the product is their square. As an example (and only as an example) with 10 specific protein niches and 10 complementary protein units available, the chances that a unit will settle can be rated at 100. If one niche has been occupied, the chances of the remaining nine, that are still bombarding the same total area, will be 9 x 9 or 81. After the second occupation, the chances for the third will be 8 x 8 or 64. Each niche that has been occupied, by virtue of that fact, reduces the chances of the remainder. Since the chances decrease by steps equal to the intervals between the squares of whole numbers, the time necessary for the process of settling should increase by corresponding increments. Our clock with constant intervals should register a declining rate of growth. Our $2t+1$-clock with its elastic shrinking intervals, should register a constant growth rate. Under conditions that have been carefully stipulated, this is precisely what happens. Thus $2t+1$, instead of being an anomaly, is a literal translation into the temporal dimension of the distinctive pattern of protein metabolism.

V

The one to one relation between tenant units and locations to be tenanted, by implication, foreshadows a geometry of growth. By definition, implicitly and explicitly, the organism is a unit. While the manifold of complex organic forms cannot easily be confined within the area covered by topological definitions, units that are not figures of speech have certain invariant properties in common. It is these that must be our subject matter. Accordingly, our geometry of growth will become no less abstract than the metrical system that is its foundation.

First and foremost are the relations of a unit entity to the space it occupies. Within this space no space is wasted. Densities in the organism, of course, differ. In the mouth are the teeth, saliva, and air. The air is continuous with the gases in the frontal sinus and antrum. In the coelom, are the "soft parts," some fluid, and some gases. In the cavities of the circulatory system, is blood. Elsewhere, we find lymph, synovial or cerebro-spinal fluid. Yet, no matter what densities we encounter, none of the spaces referred to could undergo changes in size or shape more serious than these borne by the more solid organs, without involving a complete reorganization and possibly the death of the entirety. Like the blastocoel of the invaginate gastrula, these spaces are essential internal working space. As such, it is one of the fractions whose summation is the unit organism in its spatial aspect.

In some of the spaces mentioned and in all the space not mentioned as yet, we find cells. These smaller units when not close-packed are imbedded in special substances of their own creation. These materials completely fill intercellular space and there play important roles. Cartilage and the other connectives, so-called, are good examples.

Among purely geometrical abstractions, the nearest relatives of our momentarily abstract organism, are the so-called space-fillers. Four, and only four types, are known: parallelopipeds like the cube; hexagonal prisms; rhomboidal and rhombo-hexagonal dodecahedra; and, the truncated octohedron or hexoctahedron. No matter how small the dimensions of these units, a sufficiently large number of anyone of them can fill any space however large—and fill it without interstices or vacant spaces. Since all space can be occupied by any of these systems, it follows that one system can be substituted for the other. Substitutions are also possible when fractional space is filled, for the properties of space fillers depend on shape and not size. Accordingly, the fraction of space occupied by one system of fillers can be occupied by any other system of space fillers with any desired degree of accuracy, provided we choose our units small enough.

In transforming our abstract organism into a system of space-fillers, we are free to exercise certain preferences. We can choose any form we like, but, once having chosen, we are at the mercy of that form. For our purposes, it is desirable to combine maximal complexity with the highest biological probability. This narrows us down to the hexoctahedron, known also as the cubo-octahedron or truncated cube or more widely as the tetrakaidecahedron.

The conditions under which this form materializes in nature are two-fold. Among other instances, it is found as crystals in phosphotungstic acid. (3). Theoretically, it should appear in any system whose units are ideally quantified, of proper consistency, and under adequate and uniform compression. As Kelvin (11) demonstrated, the hexoctahedron divides space with minimal partitional area. In carefully constructed foams, it is more closely approximated than any other. This is also true in many plant and animal tissues (13–16), although the ideal form is rarely realized. In heart-muscle, it is not even approximated. On the other hand, no muscle, fibrous tendon, or neural tissue is known to contain waste space. For this reason, a system of this type can be transformed quantitatively into an ideal system. The approximate systems, such as the yolk of hard-boiled eggs where the ideal form is rare but sometimes identifiable (8), are self-compensatory, and hence can be treated as if they were ideal. This is true likewise in systems of starch grains. In corn starch, the form is present either as visible units or aggregates.

At one point in the natural history of proteins, these demonstrably constitute either a self-compensatory system or else have the form of a space-filler. Half of the genetic proteins of the individual are paternal in origin. During the act of fertilization, this half is contributed by the sperm-head. From the available measurements, one of the most eminent geneticists, Muller (20), has calculated that the total volume of the sperm-heads required to produce two billion human beings equals the space occupied by half an aspirin tablet. Under these conditions, the genetic proteins in the sperm-head would appear to be slightly crowded. If they are so crowded, they are present either as a system of irregular entities self-compensatory with respect to space, or else, as units, which have the shape of aggregates of space fillers.

When we examine the genetic proteins of caterpillars infected with the virus of flacherie, all the chromatin in certain nuclei has apparently disappeared. In its stead, we find the well known "polyhedral bodies" varying in size from 0.5 to 15 microns (9). Careful optical analysis of their forms discloses the square and hexagonal aspects and the angular measurements that identify the hexoctahedron as a unit and as a symmetrical aggregate.

Model stacks, in which the individual is the unit, are symmetrical, completely self-determining, and transform into rhombic dodecahedra. The surfaces of these are orderly terraces with nests of highly specific kinds, into which units of the next shell can fit in only one way. Although there are only twelve major planes, the dodecahedron has eight hexagonal aspects and six that are square. Face-centered on each aspect is a projection of the corresponding facet of the central unit. Here are all the connexities required for tight spatial organization. It is interesting in this connection, that the 8 hexagonal and 6 square facets of the basic space filler and the corresponding projections of these are topologically identical with the cyclol cage structures postulated for active proteins, (26, 27).

When aggregates of fifteen individuals are stacked as units, the original hexoctahedral form is restored when fourteen of these aggregates have been connected. Four more transform this object into an octahedron. In these super-aggregates, the terracing is accentuated and the nests, more numerous and varied, are larger and deeper than in those in which the individual hexoctahedron is the building unit. Such radical transformations and similitudes, together with the new diversities that appear, increase the biological relevance of the original form. It is a matter of considerable interest that some evidence of terracing is now available in the polyhedral bodies of insects and that super-aggregates of the hexoctahedral units, which retain the symmetries of the unit, e.g., these super-aggregates of fifteen composite units, conform to the specifications required by the data on bushy stunt virus (2). This virus appears in the form of rhombic dodecahedra.

Thus, the picture of aggregation, whether of individual units or aggregates, has a resemblance to Wrinch's idea that the heavier proteins, including particularly the viruses, are cage colonies composed of cage structures in orderly association (28, 29). This convergence of two lines of thought should not obscure the fact that these lines trace to totally different sources.

The metrical parameters of these aggregates have a peculiar fascination. Marvin (16) was the first to publish the aggregation series in terms of individual units. The equation describing this series is easily converted into the form of the $2t+1$ growth equation (5: 8). The quantitative data on hexoctahedral aggregation can therefore be substituted for either x or y in the allometric equation $y=bx^{\alpha}$. When shell number in the aggregate agrees with interval number on the time scale, then the predicted linear relation on a logarithmic grid is verifiable remarkably well for the chick as a whole, and for its chloride complex. (8). Equally good agreement may be possible with other aggregation series, but none would be accompanied by geometrical transformations or diversifications having the significance of this one. Moreover, none of the others are equally convenient. These particular aggregates simulate the $2t+1$ law in easily recognizable form. On the surfaces of 1-, 2-, and 3-shell aggregates, the units are arranged in terraced rows. The 1-shell aggregate has three such rows; the 2-shell aggregate has five, and the 3-shell aggregate, seven. Thus, the metrical system in which these aggregates enlarge is immediately manifest. This system based on differences in the squares of whole numbers was first detected in data on the growth of the chick as an entirety (5), but it can receive universal application. If our last step is correct, the $2t+1$ law is a consequence of protein metabolism.

That hexoctahedral aggregation should simulate an organic growth rate and, that the hexoctahedron as an individual and in aggregates should reproduce forms present or approximated at the cellular level and at subcellular levels in substances as different as corn starch, the bushy stunt virus and the genetic proteins of insects, can mean only one thing. All the processes concerned in the growth of an organism have a common geometrical basis. In final detail, the geometrical and purely metrical pattern, as understood at this moment, will call for particular amendments. Nevertheless, the amended version is not likely to escape from the numerical parameters of this one, because this one has the numerical parameters of the organism.

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SECTION OF ANTHROPOLOGY

October 27, 1941

Doctor Franz Weidenreich, Cenozoic Research Laboratory, Peiping Union Medical College, Peiping, China: *The Site and the Technique of the Excavations of Fossil Man in Choukoutien, China.** (This lecture was illustrated by motion pictures and lantern slides.)

Choukoutien, where the Peking Man—*Sinanthropus pekinensis*—has been recovered, is a typical Chinese village situated at the foot of the Western Hills (Hsi Shan) on the border of the Hopei plain and a little more than forty kilometers south-west of Peiping. The place had already acquired a certain reputation before it was recognized as the burial place of *Sinanthropus*. This reputation had really some connection with fossil man, for it is from Choukoutien that lime and coal are brought to Peiping. The lime derives from limestone quarries above Choukoutien, where it is burned in rather old-fashioned kilns, while the coal is mined, near by, in a not less primitive way. Thus, there are two Choukoutiens, a white one and a black one, both exactly corresponding to the two geological formations characteristic of the Choukoutien area.

The Western Hills represent the eastern margin of a deeply dissected Late Mesozoic platform with altitudes up to 3,000 feet above sea level. Geologically, their mass chiefly includes Ordovician limestone, Triassic sandstone and Jurassic conglomerates, slates, and sandstones. The high mountainous background is formed by these Jurassic conglomerates and sandstones, while the foothills of Choukoutien contain coal in the carboniferous slates and limestone and caves in the Ordovician strata.

The mountain range and the foothills, which stretch from north to south, are dissected at Choukoutien by a short and narrow valley with an usually dry river bed running from west to east. This river separates economically, but not geologically, the white and black Choukoutiens. The foothills, south of the river, provide lime and caves, those north of it, coal.

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*The geological, stratigraphical, paleontological and historical data are compiled from earlier publications. The reader is referred in particular to the publication of the Cenozoic Research Laboratory: "Fossil Man in China. The Choukoutien Cave deposits with a synopsis of our present knowledge of the Late Cenozoic in China," Geological Memoirs, Series A, Number 11. 1933.

The Late Cenozoic deposits preserved in the Choukoutien area occur in three formations: (1) as a terrace of gravels high above the present river bed and possibly belonging to the Lower Pliocene; (2) as a sheet of dark red clays; (3) as a huge Loessic erosional fan belonging to the Upper Pleistocene.

The Ordovician limestone includes a system of pockets and fissures which in some places are of large extent in width and length. The pockets are entirely filled with sand, rolled boulders and gravels. They are not fossiliferous. The fissures contain more or less cemented deposits of red or yellow clay, breccia, etc., and, are very rich in fossils. There are also true caves, partly empty, showing, in part, walls covered with old stalactites and stalagmites. The empty caves were probably formed in Upper Pleistocene times or even at a more recent date.

The fossiliferous fissures are of special interest to the paleontologist. They become exposed when the limestone is quarried, and quarrying has apparently been practiced in Choukoutien since, perhaps, prehistoric times; for banks of waste lime as refuse of kilns occasionally form terrace-like layers above the plain and the river bed.

Sinanthropus owes his discovery, primarily, to the exposure of such a fossiliferous fissure by commercial exploitation of the Ordovician limestone and, secondarily, to the initiative of the Swedish mining engineer, Dr. Gunnar J. Andersson, who was hunting for fossil bones during and after the first World War, while in service of the Geological Survey of China. In the spring of 1918, Dr. Andersson visited the Choukoutien district. In one locality known as Chikushan (Bird's Bone Hill), there stood an isolated pillar enclosing a great number of bones of small birds and mammals. This pillar, evidently, represented the only remnant of an originally much larger deposit, the surrounding limestone walls of which had been completely broken away.

In the summer of 1921, Dr. Andersson visited the Chikushan locality, again, accompanied this time by the paleontologist, Dr. Otto Zdanski, in the service of the Geological Institute of the University of Uppsala, and our late friend, Dr. Walter Granger, who was on his way to the Gobi expedition. Quarrymen called their attention to a deposit, near by, much larger and richer in fossils than the one first discovered. This deposit was situated north-west of the railroad station of Choukoutien and at the north side of the foothill of that mountainous section that arises immediately south of the Choukoutien river and west of the great plain or, in other words, just where the river reaches the plain. This deposit is now known as Locality I and is, so far, the only site where human bones come to light. At the time of its discovery, the deposit was partly exposed at the head of an abandoned quarry. Among the loose material fallen into the quarry from the face of the deposit, Dr. Andersson noticed fragments of white quartz, a mineral normally foreign to that locality, and, at once, recognized this occurrence as an indication of the presence of fossil man.

The deposit of Locality I is, by far, the largest and richest of the fossiliferous fissures in the Choukoutien area. Fifteen of them have been excavated. They yielded very interesting faunas, some older, some younger, than those of Locality I, but no human bones. During a long period of history, Locality I must have been largely empty and cave-like. Gradually, however, through weathering action, periodical roof collapses and accumulation of detritus, its great cavity was filled with clay, sand and hard brecciated travertine.

At the time when the regular excavation of Locality I began (1926), the extension of the deposit, so far as it had then been exposed, amounted to 175 m. in a west-east direction, to about 50 m. in breadth (north-south direction), and to approximately the same dimension in height. There is, of course, no possibility of estimating the original extension in the latter direction. When the deposit was discovered, it was already exposed at its uppermost layers and did not reach to the summit of the hill. This summit was, and is still, capped by a layer of extremely hard stalagmitic travertine full of bones of bats and teeth of other mammals. The contents of this layer prove that what today appears as the cap of the hill was once the floor of a cave with bats living in it. The walls and roof of this cave apparently situated above Locality I have been carried off by natural events, long ago.

Thus, the original shape and extent of Locality I cannot yet be fully defined. Five regions can, however, be distinguished. (1) What is called the Main Deposit. This forms the central and most extensive part of the exposed beds. (2) The Lower Fissure, represented by a narrow cleft extending northwards from the Main Deposit. As it dissects the northern limestone wall in its entire height, it offered the easiest access to the deposit from the river side. (3) The Kotzetang Cave. This cave is a wide hole within the eastern mass of breccia, and obviously an artificial formation; except for the northern wall, which consists of the original limestone. A hard, cemented breccia forms its walls and roof. What has brought about the formation of the cave is unknown, but it seems the result of excavations made, perhaps, by prospectors searching for workable limestone. (4) The Eastern Slope. This is the face of the hill which inclines eastward toward the plain. The surface of which consists of the same breccia that constitutes the Main Deposit. (5) The "Upper Cave." Its entrance is on the north face of the hill high up and near the top. The cave represents a hole in the Loessic formation, its walls partly covered with old stalagmitic incrustations. Downward, however, the cave extends somewhat into the deposit proper of Locality I.

Before describing the history and technique of the excavations at Locality I, some words may be said of the geological period to which its fossiliferous deposits have to be attributed. That they are Late Cenozoic has been already mentioned. To define the age more precisely, the stratigraphical facts, as well as the fauna recovered, indicate that the deposits fit in between the Late

Pliocene, at one extreme, and Upper Pleistocene, at the other. Consequently, they must be considered broadly as of Lower Pleistocene Age. More accurately, they are younger than the red clay formation at the third sedimentary cycle in North China which is characteristic of the Sanmenian, or, Late Pliocene period, but older than the Chinese Loess and the Mongolian sands which characterize the fifth sedimentary cycle of the Upper Pleistocene. Corresponding to this stratigraphic evidence, the fauna is determined by the absence of *Hipparion*, characteristic of the Late Pliocene strata in China, but at the same time, also, by the absence of *Bos primigenius* and red deer, both characteristic of the Upper Pleistocene strata. On the other hand, saber-toothed tiger, *Hyena sinensis*, *Rhinoceros mercki* and muskox, all found in most of the red clays of the Late Pliocene of China but absent in the Loess of the Upper Pleistocene, are still present in Choukoutien.

Excavations in Locality I were carried out first in the summer of 1921 by Dr. Otto Zdanski as paleontologist, and, again, in 1923. The fossil material collected then was shipped to Professor Wiman's laboratory in Uppsala, where it was prepared and studied. In 1923, Dr. Zdanski recovered a worn and fossilized molar tooth among this material and recognized it as human. A second tooth of the same kind was later found and, also, sent to Uppsala for preparation. When these discoveries were known in Peiping, the National Geological Survey of China, in cooperation with the Department of Anatomy at the Peiping Union Medical College and supported by a grant from The Rockefeller Foundation to the China Medical Board, organized a research program for an intensive and systematic investigation of the Choukoutien deposits.

Dr. Davidson Black became Acting Director of the Cenozoic Research Laboratory founded especially for this purpose, and the Swedish paleontologist, Dr. Birger Bohlin, took charge of the paleontological field work. In April, 1927, the work began in its new form. Three days before the spring excavations were stopped, because of the rainy season, an additional human molar tooth was recovered by Dr. Bohlin himself. The morphological character of this tooth induced Dr. Black to give the hominid type so verified for Choukoutien the special name of "*Sinanthropus Pekinensis*," as suggested by the paleontologist of the National Peking University, Dr. Amadeus W. Grabau. During the following two years, several lower jaws, isolated teeth and a badly crushed parietal bone, were recovered. In December, 1929, just before the excavations had to be stopped on account of the approaching winter, Dr. W. C. Pei found an almost complete cranium of *Sinanthropus* (skull of Locus E) with only the face missing.

From their opening in 1927, the excavations went on, except for seasonal intermissions, up until the summer of 1937. Three days before the Japanese began the war with an attack near the Marco Polo Bridge, on the way between Peiping and Choukoutien, the work had been stopped, as is usual with the first days of July. On the very last working day, a well preserved fragment of an upper jaw and two isolated teeth of two more individuals were recovered. All the material stored up in the field headquarters in Choukoutien during the working season could still be safely brought to Peiping. In all these years since then, however, there has been no possibility of resuming the excavations.

The excavation of the "Upper Cave" was carried out in 1933 and 1934. It yielded an unimaginable wealth of skeletons of animals: hundreds and thousands of entire skeletons, or parts of them, of tiger, bear, deer, hyena, hare, etc., all belonging to the Upper Paleolithic. In addition, there were found, within a restricted area of the cave, skeletons of man with the morphological characteristics of modern mankind and, obviously, belonging to the category of Upper Paleolithic Man. Among the human bones were three well preserved skulls and fragments of four more, probably representing the remains of one family.*

The *Sinanthropus* bones and teeth recovered in Locality I, during the period of ten years, belong to about 45 individuals, adults and children, males and females. They consist of 14 skulls and skull fragments, fragments of 14 diverse lower jaws, all of them belonging to individuals different from those represented by the skulls, 146 teeth in situ or isolated, 7 fragments of femur, 2 fragments of humerus, 1 wrist bone, 1 collar bone, and 1 fragment of an atlas. All the bones were broken and scattered over the whole deposit from top to bottom. Intermingled with these human remains, there were recovered thousands of animal bones, most of them fragments and broken in the same way as the human bones. There were, furthermore, collected thousands of stone implements, whole layers of seeds of hackberries, pieces of charcoal, layer of ashes, coprolites, in particular of hyena, burnt implements and burnt bones.

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*Recent literature on the "Upper Cave:"

Pei, W. C., The Upper Cave industry. Palaeontol. Sin., New Ser. D, No. 9, pp. 1–58, 1939.

Pie, W. C., The Upper Cave fauna of Choukoutien. Palaeontol. Sin., New Ser. C. No. 10, Whole Serv. No. 125, pp. 1–100, 1940.

Weidenreich, Franz, On the earliest representatives of modern mankind recovered on the soil of East Asia. Peking Natural Hist. Bulletin, 13. pp. 161–174, 1939.

In order to collect and preserve all these manifold things and to determine eventually more closely existing relations between them, it was necessary to proceed with every possible care and, in particular, to mark in each case, the exact site where the finds had been picked up. To secure undisturbed and continuous execution of the work, the entire hill containing the deposits has been purchased for the Cenozoic Research Laboratory and a house was built on the premises with accommodation for the scientific staff of the laboratory when working in Choukoutien.

The excavations carried out by specially trained Chinese technicians are done chiefly with small picks and hooks. If the travertine blocks are too large, they are blasted with ordinary gun powder lest the explosion be too violent and rend the bone embedded within the matrix. In sandy, or loamy layers, only hooks are used. Every bone, bone fragment or tooth, however small, is picked up and put aside in a basket which each technician has ready for this purpose. A group of technicians always works together, so that practically each lump of earth will be scrutinized. Nevertheless, the loose earth, too, is afterwards transported to a special place and passed through a fine sieve. Blocks or hard breccia are cautiously broken into small pieces and, in case they contain bones, are also put aside in baskets.

In order to record the exact location of the finds within the large extent of the deposit, the excavating is carried out according to the following rules. First, the entire floor is leveled and then divided in squares drawn with white lines directly on the floor, the longitudinal ones running from west to east and the cross-lines from south to north. The lines keep a distance of one meter from each other, so that each square measures one square meter. The squares are designated by letters and numbers. The first series begins at the western border with the letter A, then follows the letter B, and so on. All the middle squares in the east-west direction have the number O; those south of them, the number +1, +2, etc.; and those north of them, the number −1, −2, etc. The excavating keeps strictly to these squares. Each group of technicians digs out the earth within four squares allotted to them down to a depth of one meter, while the partitions between the large squares, resulting from this procedure, are left intact as long as possible. All the material collected within one square is deposited in a basket, which carries letter and number of the square concerned. When the baskets are full, they are brought to the packers. The packers first protect the eventually exposed and broken bones with covers of plaster and rice paper, wrap each specimen in paper and write letter and number on the wrapping. Then the material is stored and shipped to Peiping from time to time.

A similar practice of designation is used to record the level the finds are coming from, that is, their locations in a vertical direction. The levels are designated by numbers and the lines marking them are painted in white on the surrounding walls together with the longitudinal division of the floor. The distance between two horizons is, also, one meter. Level number, together with the date of the recovery, is recorded on the wrapping of the specimens. In this way, it is possible to reconstruct by horizontal and vertical plans the exact locations of the finds.

At the end of the excavation season, the technicians prepare the recovered specimens in the main laboratory in Peiping. Bones which have been recognized on the spot as human are sent immediately to Peiping.

As mentioned above, there has been no possibility of resuming the excavation work in Choukoutien since July, 1937, because of the political situation. Since several important finds were made the very last day we stopped the work, it is certain that we shall be successful when we may begin again. The extension of the deposit, still waiting for excavation, is approximately as large as the part already excavated. Of course, however, it is impossible to tell when the deposit will be exhausted. Nobody is able to make any prediction in this respect. As we know that there was once a cave on top of the hill, which has completely disappeared except for the bottom, it may be that we shall come across another cave, somewhere, below the one we are excavating now. There is still another possibility. The examination of the material derived from the travertine cap showed this deposit to be not younger than the main deposit, as was supposed, but evidently older and apparently belonging to Late Pliocene. This fact opens the prospect of possible finds within the area proper of Choukoutien belonging even to the Tertiary.

SECTION OF PSYCHOLOGY

OCTOBER 20, 1941

DOCTOR FRANK A. BEACH, American Museum of Natural History: *Instinct and Intelligence.* (This lecture was illustrated by motion pictures and lantern slides.)

This evening, I am going to concern myself with the problem of the relationship between instinct and intelligence. I shall outline an hypothesis bearing upon this relationship, describe the line of reasoning which has given rise to the hypothesis, and report an experimental test of the validity of the hypothesis.

THE HYPOTHESIS

Our first step must be to outline the hypothesis which is to be put to experimental test.

Study of the behavior of many species of animals has revealed two facts of basic importance to the subject we are considering. The first fact is that members of the same species differ markedly in their intelligence in so far as intelligence is reflected in the available tests. The second fact is that individuals of the same species display great differences in the efficiency with which they execute complex patterns of instinctive behavior.

Differences in learning ability or intelligence may be illustrated by the range of error scores made by a number of rats tested in the same maze, by the differences in average length of time necessary for several monkeys to solve a problem, or by the differences in the I.Q.'s of human beings. Variations in instinctive behavior appear when we study the courtship, the mating behavior, or the maternal responses of many individuals in any one species. For example, some chimpanzee mothers with their first offspring appear bewildered and at a loss to know what to do with the infant. Other inexperienced mothers of the same species are much more efficient in the care of their young. Similar differences appear in more striking form in the case of the rat. A high percentage of inexperienced female rats are able to care for their first litter quite as efficiently as an experienced mother. A few individuals, however, do not build a good nest before the arrival of the young, fail to clean the young as they are born, collect only a part of the litter in the nest, etc.

The hypothesis which I would like you to consider is this: Perhaps within a species the individual differences in intelligence are positively correlated with the individual differences in inherited behavior. It is possible that the efficiency with which an inherited response is executed depends in part upon the intelligence of the individual.

Development of the Hypothesis

Logical development of the subject leads us next to a consideration of several lines of evidence which seem to support the plausibility of this hypothesis.

Evolution of Behavior

Comparison of the behavior patterns displayed by animals at various levels of the phylogenetic scale indicates that, in the course of vertebrate evolution, three major changes have occurred in behavior. First of all, it may be observed that, in a general way, modifiability of behavior increases with evolutionary advancement. The simplest animals are not incapable of learning, but their ability to acquire new modes of response as a result of experience is distinctly limited in comparison to that of the higher living forms.

In the second place, it appears, on the whole, that the proportion of an animal's behavior repertoire dictated by inherited mechanisms is inversely related to the animal's position on the phylogenetic scale. Instinctive activities. forms of behavior rigidly controlled by inherited factors, comprise a very large part of the repertoire of lower forms. Study of the behavior of intermediate and higher groups reveals that less and less of the sum of the animal's potential reactions depend exclusively upon inherited organization.

Thirdly, it should be noted that such instinctive patterns as do persist throughout a large portion of the evolutionary scale become more and more flexible and modifiable as the phylogenetic position rises. In mammals, for example, certain patterns of response appear definitely to be inherited; but in each individual these reactions are subject to change and adaptation as a result of individual experience.

EVOLUTION OF THE BRAIN

The significance of this evolutionary trend in behavior becomes apparent when we consider its relation to a concomitant series of changes in the brain. The evolution of the vertebrates has included acquisition and elaboration of new brain areas. Today, phylogenetic position is associated with the amount and complexity of the forebrain (*Illustrated by slide*).

The lower vertebrates possess small and relatively simple forebrains. Passage up the scale includes a rapid increase in the absolute and relative size of the forebrain. Finally, in the higher mammals, the cerebral cortex, which is the most recently acquired and complex portion of the forebrain, comprises 70 per cent of the entire central nervous system.

THEORIES OF FOREBRAIN FUNCTION

Observation of the two evolutionary trends which we have mentioned has led some students to suggest that the older parts of the brain control the older forms of behavior. That is to say, instinctive reactions depend exclusively upon the functioning of centers in the brain stem. This theory further states that the newer parts of the brain are concerned exclusively with modifiability and learning; or, the cortex functions only when the animal learns new responses through experience.

This line or reasoning receives support from experiments and clinical tests showing that removal or destruction of various regions and amounts of cortex is often followed by reduction in the ability to learn new reactions. For example, rats or monkeys or men deprived of varying amounts of cortical tissue are less able to solve new problems than are normal individuals of the same species.

However, there is evidence (*Slides shown here*) indicating that destruction of cortical tissue also interferes with the execution of instinctive behavior. This fact may be illustrated by a motion picture comparing the maternal behavior of two female rats with their first litters. One animal is normal and the other has been deprived of approximately one-third of the cerebral cortex. (*A moving picture illustrating these rats was presented*).

We now have at hand several related facts. First, the evolutionary appearance of the cortex is associated with increased modifiability of behavior, learning ability or intelligence. Second, experiments with animals possessing a well developed cortex and, at the same time, retaining complex patterns of instinctive behavior have shown that the cortex is intimately concerned in the efficiency with which those inherited responses are carried out. Third, in these same animals, there exist marked individual differences in learning ability, and equally great variations in the efficiency of instinctive performance. These facts lead to the hypothesis presented at the beginning of my talk—namely, that, within a species, individual differences in the efficiency of instinctive behavior and individual differences in learning ability or intelligence are positively related.

EXPERIMENTAL TEST OF THE HYPOTHESIS METHODS

In an attempt to determine whether there is any relationship between learning ability and efficiency in inherited behavior, a simple investigation was conducted. The experimental subjects were 40 virgin female rats without previous experience in learning tests.

Each animal was given 30 trials on a simple maze and the total number of errors was taken as an index to the individual's learning ability. Efficiency of inherited behavior was gauged by the results of a battery of maternal tests involving quantitative measures of the following activities: (1) amount and kind of nest-building before parturition, (2) care of the litter at time of delivery, and (3) retrieving of young scattered about the cage by the experimenter.

RESULTS

Comparison of maze scores with records of maternal behavior revealed that those females which were the best learners on the maze made the highest scores on maternal tests, and those females that were very poor maze learners ranked among the less efficient mothers. Furthermore, a positive relationship between performance in the various tests of maternal behavior was revealed by the fact that females ranking above the average on one test (such as nest-building) tended to rank above the average on all other tests. (*These various interrelationships were illustrated by slides.*)

CONCLUSIONS

The positive correlation between maze learning and maternal performance is far from perfect, and it is probable that the relationship depends upon several factors in addition to those investigated in our study. Nevertheless, the results of this experiment appear to be in harmony with the hypothesis that the efficiency with which an inherited behavior pattern is executed is, in part, a function of the intelligence of the individual.

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ADDED TO THE ROLLS DURING THE SUMMER RECESS FROM APRIL 24 TO OCTOBER 6, 1941

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TRANSACTIONS
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SECTION OF BIOLOGY*

NOVEMBER 10, 1941

SYMPOSIUM ON ALLERGY

BRET RATNER AND HAROLD A. ABRAMSON

I

DOCTOR BRET RATNER, New York University College of Medicine, New York, N. Y.: *The Nature of Allergy.* (This lecture was illustrated by motion pictures.)

At the dawn of the twentieth century, horse serum in the form of diphtheria antitoxin was just beginning to be used. Practically all children who received such antitoxin developed, after a week or more, a condition to which Von Pirquet gave the name "serum krankheit." The symptoms presented were urticaria, angioneurotic edema, temperature and certain blood changes. The condition lasted for a few days and, in most instances, passed off without untoward results. However, Pirquet noted that, when repeated injections were administered to these same children at subsequent times, some developed asthmatic seizures along

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*The Section of Geology and Mineralogy met November third. No abstract was received from Doctor Paul F. Kerr, who gave an address entitled, "A Brazilian Reconnaissance."

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with the symptoms mentioned above, which came on precipitously within a short interval after the injection. He realized that he was dealing with a new entity. Somehow these children were changed by their first injection and they reacted in an altered manner to subsequent injections.

Because the term which Pirquet coined "allergy" has been popularized, I have cited his work first. But, a few years prior (1902), a French physiologist, Charles Richet, made the following remarkable observation. He was experimenting upon dogs with an extract of certain sea fauna. He injected one particular dog, whose name was Neptune. This dog was unaffected by the first injection. Professor Richet then went off on a holiday. Upon his return, some 22 days later, he again injected Neptune with the same extract. To his amazement the dog rapidly went into shock, had a hemorrhage from the bowels, vomited copiously and died within a few minutes. He had injected many animals prior to this, but never before had such a thing happened. He retraced his steps and came to the realization that he had fallen upon the discovery of a heretofore unsuspected biological phenomenon, namely, that an animal that is injected with an otherwise harmless substance is changed somehow in his reaction to a subsequent injection of this same substance. Richet called this phenomenon anaphylaxis.

That Pirquet did not credit Richet with his discovery and adopt the term anaphylaxis for the phenomenon in the human being was a source of great unhappiness to the French physiologist. In the voluminous literature that has stemmed from these original discoveries, his failure to do so has given rise to a great controversy which still is unsettled. Are anaphylaxis in the animal and allergy in man identical phenomena? The controversy still rages and, while I, personally, feel it has impeded our progress in this field, nevertheless we have moved on.

It is probably fitting, before showing the film, to recall in passing that Richet, in his altogether delightful volume "French Savants," remarked that one can work a lifetime on a problem and not contribute anything of significance, and yet make a discovery of great importance after working on another problem for but a short period. Thus was anaphylaxis discovered, a phenomenon in biology which bids fair to take its place amongst the great discoveries, and for which Richet deservedly got the Nobel Prize.

With these all but too few remarks, I should like now to present my moving picture, which may help to correlate the observations thus far made in man and animal.

The following is a synopsis of the cinema as it unfolds on the screen:

The entrance of harmless foreign proteins (antigens) into the body of man or animal may give rise to a state of hypersensitiveness. The initial entrance of a foreign protein produces no ill effects. Following an incubation period of at least 10 days, a second entrance or reinjection of the same protein may no longer be innocuous, but may produce severe reactions. Because it is capable of inducing a state of hypersensitiveness, the *primary* contact is known as the *sensitizing dose*. The *secondary* or subsequent contacts are known as *shock doses*. The terms most commonly used to describe the phenomenon of protein hypersensitiveness are *anaphylaxis* and *allergy*. *Anaphylaxis* means "without protection" and usually refers to the shock reactions in experimental animals, but may also be used to denote severe reactions and death in man. *Allergy* means altered "reactivity" and, in man, encompasses a large variety of hypersensitive states and syndromes clinically manifested in the form of *asthma*, *hay fever*, *eczema*, *urticaria*, *angioneurotic edema*, *migraine*, *gastro-intestinal disturbances*, and possibly other disease entities. Allergy is the more inclusive term, for it not only includes anaphylaxis and the allergic diseases, but also a variety of conditions found in tuberculosis, scarlet fever, rheumatism, and other infectious diseases. Knowledge of allergy depends, probably more than that of any other malady, on animal experimentation.

ANAPHYLAXIS IN EXPERIMENTAL ANIMALS

I. The Guinea Pig. Normal guinea pigs are given a sensitizing injection of a foreign protein by the intraperitoneal and subcutaneous routes. They remain normal. Two weeks later, sensitized animals are given a second or shock injection of the same protein by the intravenous route. Mild reaction. Lethal anaphylaxis. The pathology of anaphylaxis in the guinea pig is essentially a tetanic spasm of the terminal bronchioles which results in emphysema. Comparison with the normal lung. A diagram of the bronchial tree (shown on the screen) illustrates the pathology of anaphylaxis. The spasm of the terminal bronchiole is the direct cause of the symptoms. The explanation of this phenomenon lies largely in the *cellular theory*, which states that, after an antigen has entered the body and a suitable incubation period has elapsed, specific anti-bodies are formed and become fixed to tissue cells. Like Ehrlich's graphic hypothesis of immunity, the following symbolism enables us to grasp somewhat the concept underlying hypersensitiveness. It is admittedly hypothetical and, from the physicochemical viewpoint, inadequate. This represents a smooth muscle cell. When antigen enters the blood stream, it remains there for a short time and then disappears. After a period of 10 days to 2 weeks specific antibodies are presumably attached to the smooth muscle cells. When the specific antigen again enters the blood stream a union occurs between it and the fixed antibodies of the unprotected cell. This union of fixed antibodies and antigen initiates the sequence of events which results in the characteristic allergic response. The validity of this theory is greatly fortified by the fact that the uterine muscle of a sensitized animal *when isolated from the organism* still shows a definite allergic reaction to the specific antigen. The following kymogram shows the effect of the antigen on the normal and regular contractions of the uterine strip. *The animal was sensitized with egg white* two weeks previously. 1. Minute markings on kymograph tracing. 2. Normal uterine irritability. 3. Normal uterine waves. 4. Milk added to bath—no reaction. 5. Egg white added to bath. 6. Allergic uterine contraction. 7. Height of contraction. 8. Tetanic spasm. 9. Relaxation of muscle. 10. Fresh Locke's solution added. 11. Egg white added to fresh bath. 11A. No reaction—desensitized muscle. 12.

Histamine added. 13. Non-specific uterine contraction. 14. Relaxation. The same action that causes the tetanic contraction of the uterine muscle, lasting for several minutes, is sufficient to cause the death of an animal when the *bronchioles* are constricted. The physiological pathology of anaphylaxis depends entirely upon the contractility of smooth muscle, and the specific symptoms of anaphylaxis will vary from species to species. The predominating symptoms in each species are accounted for by the location of the largest amount of smooth muscle present in the different vital organs; e.g., the *bronchioles* in the guinea pig, the *pulmonary artery* in the rabbit, the *hepatic venules* in the dog.

II. The Rabbit. A normal rabbit is given an intravenous injection of a foreign protein and shows no reaction. A sensitized rabbit is given a shock injection. The symptoms are mild and are due to a constriction of the pulmonary artery. It is difficult to induce severe anaphylaxis in the rabbit. The rabbit develops a higher precipitin titer than any other animal, which can easily be demonstrated by the precipitin test. A normal guinea pig readily can be passively sensitized with the blood of a sensitized rabbit. If a rabbit is given subcutaneous injections repeatedly at several day intervals he will develop a local inflammatory reaction in the skin and underlying tissue, known as the *Arthus phenomenon.* This phenomenon has been reported in the human being after repeated injections of serum and may sometimes result in severe local necrosis.

III. The Dog. Dogs were given intravenous injections of 10 cc. of a foreign protein (horse serum), followed three days later by a subcutaneous injection of the same protein. Three weeks later, these dogs are given an intravenous shock injection of 20 cc. The animals show symptoms of anaphylaxis ranging from a mild degree to severe shock terminating in death. Note the retching, vomiting, hemorrhage from the bowel, and complete collapse.

The anaphylactic symptoms were due directly to a constriction of the *hepatic venules.* The first animal recovered and the last died in complete collapse with continued hemorrhage from the bowel (death occurred two hours later). The liver, removed from the animal which died, weighed 485 gms. Contrast this with a liver removed from a dog of the same size dying of other causes: weight, 130 gms. The enlargement is due to a stasis of portal blood. The sequence of events in canine anaphylaxis can be visualized with the aid of the following diagram. In addition to the blood pressure changes, at the height of anaphylaxis, there are chemical changes in the blood indicated by the loss of coagulability. Blood drawn from the sensitized dog before the shock injection coagulates in several *minutes.* Blood taken from the same dog at the height of anaphylactic shock does not coagulate even after several hours.

Immunity Phase of Hypersensitiveness

When an animal has recovered from anaphylactic shock, it will resist a subsequent injection of the specific antigen. This phase is known as *anti-anaphylaxis.* When an animal has received small daily injections of a specific antigen *without an incubation period,* it will also be refractory. Sensitized animals which receive frequently repeated small injections of the specific antigen may become *desensitized.* These immunity phases are temporary and relative, depending largely upon the quantitative relation of antigen to antibody. A graphic representation of what probably occurs during these periods. During antianaphylaxis and desensitization, antibodies remain fixed to the cells. They are also in excess in the circulation. If antigen enters the blood stream during these periods, it is neutralized by the excess antibodies in the *circulation* and is prevented from reaching and combining with the antibodies in the smooth muscle cells. The proof that human allergy is due to an antigen-antibody complex is furnished by the Prausnitz-Küstner reaction. Prausnitz-Küstner reaction: Step 1—Blood is drawn from an *allergic* child. Prausnitz-Küstner reaction: Step 2—The serum separated from this blood is injected *intradermally* into a *normal* adult. Prausnitz-Küstner reaction: Step 3—48 hours later the protein, to which the allergic child was sensitive, is injected intradermally into the passively sensitized area of the recipient. A positive reaction occurs *in this area,* but not elsewhere. This passive sensitization of a localized area in the skin of a normal individual is due to the presence of specific antibodies in the blood of the allergic individual.

NATURALLY ACQUIRED HYPERSENSITIVITY

The hypersensitivity of man does not arise exclusively from injection. A protein may enter the body through a *natural portal,* and function as though it were injected. The ways in which a human being may become sensitized can be demonstrated in the guinea pig.

Experimental Asthma. A normal guinea pig is placed for several hours in an atmosphere containing dry horse dandruff. There is no reaction. Three weeks later guinea pigs that have inhaled dry horse dandruff dust for several hours are returned to this same dust environment and show shock symptoms. Sensitization and shock manifestations in these animals were induced solely through the medium of inhalation, showing different degrees of hypersensitiveness. The death of the animal is due to the tetanic spasm of the bronchioles, for the *heart* remains beating several minutes after asphyxia. If an animal is sensitized by the inhalation of horse dandruff, it may die from a *primary injection* of horse serum *antitoxin.* Asthmatic human beings sensitive to horse dandruff may, in like manner, die suddenly after a primary injection of horse serum.

Food Allergy. Normal guinea pigs are fed, for the *first* time, a harmless natural product—milk. No reaction ensues. Two weeks later, when animals, sensitized by feeding, are fed milk for the second time, anaphylaxis may result. About 50% of animals can be sensitized through the medium of feeding. The reactions are usually mild, but infrequently may be severe or fatal.

Fatal anaphylaxis in human infants, following the ingestion of milk, has been reported. These reactions occur with *native* food proteins. When such proteins are *denatured* by heat or digestion they may lose their antigenic properties. For example, *whey proteins* of milk are *most often* responsible for milk idiosyncrasy. Intensive heat treatment under controlled conditions, will denature *whey* proteins so that such milk can be tolerated by a milk sensitive individual. A guinea pig sensitized to *pure lactalbumin* resists a shock injection of denatured milk; but when this is followed by an injection of *undenatured* raw milk the animal dies in anaphylactic shock. No reaction with denatured milk. Death from raw milk (same animal). Malt extracts contain antigenic barley proteins. Carbohydrates derived from malt extract lose their antigenic properties in the process of preparation. An animal sensitized to *malt extract* is given a shock injection of a 10% solution of *dextri-maltose* and shows *no reaction.* When this is followed by an injection of a 10% solution of malt extract anaphylaxis occurs. No reaction with dextri-maltose. Death from malt extract (same animal).

Sensitization in Utero-Congenital Allergy. If pregnant animals are given either injections of horse serum, or feedings of milk, or inhalations of horse dandruff, their offsprings may become sensitized *in utero.* A newborn, sensitized *in utero,* may show an anaphylactic reaction on first contact with the substance to which it was sensitized. This sensitization *in utero* may be *passive* or *active.* Guinea pigs born of normal mothers show no reaction. A guinea pig, "actively sensitized *in utero,*" is shown to react to its initial contact with horse serum when three weeks old. This experimental demonstration of sensitivity, congenitally acquired, lays the basis for an understanding of certain so-called inherited types of hypersensitivity in human infancy. In these instances, the infant is either actively sensitized to a food protein the pregnant woman has ingested; or, the infant is passively sensitized to the antigen to which the allergic mother reacts.

HUMAN ALLERGY

Approximately 10% of the population suffer from the major allergies—asthma, hay fever, allergic eczema, or allergic urticaria. A much larger proportion of the population probably have minor allergic manifestations, which would also include certain drug idiosyncrasies. The generally accepted viewpoint is that allergy is inherited. As demonstrated, however, specific hypersensitivities can be naturally acquired by the individual. The diagnosis of allergic diseases depends to a large extent on a careful history, a complete physical examination, and a thorough differential diagnosis to exclude non-allergic conditions which simulate these diseases. Once it is determined that the patient is allergic, the physician must next embark upon the search for the protein or proteins directly responsible for the allergic symptoms. The specific offender can often be discovered by careful observation and by the use of elimination diets. However, the truly great advance in recent clinical knowledge of the specific causes of allergic diseases has been made by the application of the protein skin tests. Two methods are available: (1) the intracutaneous, and (2) the scratch. The latter method is preferred, particularly in children. A positive skin reaction is one which usually occurs within a few minutes and assumes the form of a wheal with erythema. Like all other laboratory methods the skin test has its limitations: (1) All patients do not give positive reactions even if they are sensitive; (2) Reactions may be positive at one time and negative at another; (3) Reactions may vary at different sites; (4) The interpretation of a positive reaction is not always easy; (5) Delayed reactions may sometimes occur. The larger the number of proteins available for application the more exact will the diagnosis become. Demonstrations of the scratch test: dried proteins are dissolved *in situ* with N/10 NaOH, at the site of the scratch. Treatment is based on the specific protein sensitivity.

It is accomplished in one of the following ways:

1. By a complete elimination of the offending protein from the diet or the environment. This is often a more difficult task than is apparent. Frequently foods, in denatured form, may be used to augment the diet and not deprive the individual of certain important food elements.

2. By establishing a tolerance for the offending protein by *oral desensitization.* Small amounts of food are given in gradually increasing quantities until tolerance is established. This may require from 6 to 9 months.

3. By establishing a tolerance for the offending protein by

parenteral desensitization. Injections are given subcutaneously. The dilution of the protein must be high enough not to produce any reaction in the patient. The increases are very slow and gradual. Great care must be exercised in the quantity administered as severe shock reactions—sometimes fatal—are known to have occurred following the injection of a quantity exceeding the tolerance of the patient.

For the desensitization of the hay fever sufferer, in which the injection method is most often used, there are two choices:

1. The "pre-seasonal" method, i.e., the injection of pollen extract at intervals for several months immediately preceding the allergic season.
2. The "perennial" method, i.e., injecting at regular intervals throughout the year.

The perennial method is the one of choice. Once more the great caution that should be exercised when any foreign protein is injected into a human being should be emphasized, for the very same symptoms that were observed to occur with the animal can occur in the human allergic. Besides the typical guinea-pig type of anaphylactic death in man, there has been a report of an individual who died from a tetanus antitoxin injection who showed all the symptoms and pathology noted in canine anaphylaxis. Serum administration of any kind and injections of foreign proteins such as milk, insulin, and others, must all be given with circumspection. The individual should be previously tested to avoid anaphylactic shock. Animal experimentation has thus thrown light on a large group of human ailments in the domain of protein hypersensitiveness and has established the basis for diagnosis and treatment.

Doctor Harold A. Abramson, Columbia University and The Mount Sinai Hospital, New York, N. Y.: *The Nature of the Ragweed Pollen Allergen.*[1] (This lecture was illustrated by lantern slides.)

II

The most common manifestations of constitutional allergy in the United States are hay fever and its frequent complication, asthma. To the pollens of the ragweeds and of the grasses may be assigned the most important etiological role in so far as the production of sensitization and tissue reaction in susceptible individuals are concerned. The tiny pollen granules are breathed in through the respiratory tract. During their travels in the nasal and bronchiole tubes of sufficiently sensitized persons (or animals), the pollen collide with the moist mucous membranes and release the active agents, the allergens, at the site of contact. These allergenic substances then pass through the mucous membranes into the underlying tissue where a tissue reaction occurs leading to dilatation of the minute blood vessels and transudation of plasma-like material. In addition, the smooth muscle-cells of the bronchioles may respond with the common phenomenon of spasm. It is evident that the exact nature of the molecules which produce these symptoms is of the utmost significance if the mechanisms of the processes involved are to be understood.

It is always of interest to describe the first experiments which led to the development of any investigation. The first experiment which was performed in this series of studies on the nature of the ragweed pollen allergen was an attempt to transport it electrophoretically into the skin of a medical student who was sensitive to ragweed. Much to my surprise, using the negative pole, a large hive with pseudopods formed at the point of application of the ragweed solution. No reaction occurred in individuals not sensitive to ragweed pollen. This was noteworthy, not so much because of the fact that an electrical field could drive substances into the living human skin but because materials presumably of high molecular weight and protein in nature readily penetrated the epidermis and reached the blood vessels in the dermis where the allergic reaction occurs. It was even more unexpected that both the positive and the negative poles were able to introduce the biologically active constituents of ragweed pollen extracts. There was no obvious reason why this should occur. In the case of giant ragweed pollen extracts, the positive pole was more efficacious. In the case of dwarf ragweed pollen extracts, the negative pole proved more efficacious. The fact that biologically active materials may be introduced in this way has been used in the therapy of hay fever both coseasonally and preseasonally.

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[1] This investigation has been aided by a grant from the Josiah Macy, Jr. Foundation.

Similar phenomena were found for timothy pollen extracts.

The fact that the allergens of ragweed and timothy pass through as relatively impermeable a membrane as the intact human skin, makes it simpler to visualize why they readily pass through the mucous membranes of the lungs and the nose. Just why they are apparently able to do this will be disclosed shortly.

Cooke, Stull and their co-workers have shown that the nitrogen precipitated by phosphotungstic acid is an important measure of the biological activity of ragweed and other pollen extracts. These workers have also found, by chemical precipitation technics, two fractions with important immunological properties in ragweed pollen solutions. Furthermore, by the use of dialysis methods and ultrafiltration, Spain and Newell as well as others have emphasized that the colloidal fraction of pollen extracts is, in all likelihood, connected with the measure of their therapeutic efficiency. The work of the writer with Sookne and Moyer also support, the view subscribing to the protein nature of the biologically active agent of ragweed pollen extracts. These investigators found that ragweed extract could be inactivated by adsorption with inert surfaces in very much the same way that serum albumin, egg albumin, and other proteins are adsorbed. Thus, quartz particles inactivated dilute ragweed pollen extract. The electrical properties of the adsorbed film obtained from ragweed pollen extracts were investigated by the microscopic method of electrophoresis.

In view of the fact that crude extracts were employed to obtain adsorption films of the active principle, it was necessary to determine that the constituents adsorbed would be taken out by the adsorbing surface independently of the nature of the surface. For this reason, typical hydrophilic and hydrophobic surfaces were compared in the following way. Electric mobility measurements were made on solutions containing increasing quantities of pollen extracts in 0.02 M phosphate buffer. It was found that the electrical mobilities of mineral oil droplets, collodion particles, and quartz particles in the presence of an excess of pollen extract were the same. This meant that essentially the same film was on these very different surfaces. When quartz particles which had been treated with pollen extract were exposed to varying concentrations of acid, reversal of the sign of charge occurred near pH 4.0 which indicated that the adsorbed film was amphoteric and probably protein-like in nature but not necessarily so. Further experiments on the electrical mobility-pH curve indicated the protein-like nature of the surface, but the electrical mobilities in alkaline solutions were very low with a practically flat mobility-pH curve. The titration curve of a sample of dialyzed ragweed extract also acted as an amphoteric solution reacting with both acids and bases approaching maximum acid combining capacity at approximately pH 2.6. On the basis of these adsorption and electrical experiments on the ragweed pollen material, we were quite ready to believe that we were dealing with a film very much like a protein but with an unusually low and flat mobility-pH curve on the alkaline side of the isoelectric point.

Although the experiments cited in the foregoing shed some light on the nature of the material, it was evident that fractionation of the deeply colored extracts could probably best be achieved by electrophoretic separation employing the moving boundary method. These experiments were begun in the summer of 1938 with J. Gagarin and L. Jennings at the Biological Laboratory, Cold Spring Harbor. Using a conventional U-tube, it was observed that the pigment was negatively charged and that the impure fast moving pigment was skin reactive. During that year, Gagarin with the collaboration of H. Gettner ran a large number of experiments with the same equipment and noted that some of the colorless material left behind by the migrating pigment was also highly skin reactive. On the basis of these fairly crude experiments with the simple U-tube, then, it was found that biological activity was present both in colorless and pigmented fractions. However, we were unable to obtain any quantitative data on the electrical mobility of the colorless fraction, on the number of pigments, on the types of molecules concerned, or on the homogeneity of the fractions. At this point, the collaboration of D. H. Moore was enlisted and the Tiselius apparatus at the Electrophoresis Laboratory of Columbia University was employed. The use of the electrophoresis equipment and the ultracentrifuge by Dr. Moore has made further important progress possible. The optical method first invented by Foucault for correcting telescope errors and adapted by Toeppler, Lamm and Tiselius for observations of refraction gradients has simplified the problem to the point where an ultimate solution seems likely—indeed most probable.

With ordinary Visking tubing and collodion membranes, we were unable to prevent loss of large amounts of highly active allergenic material. With dried collodion membranes, the diffusion of the allergen could be retarded, but these membranes also prevented the diffusion of salt to a great extent. After many unsatisfactory experiments using dialyzed solutions in which low concentrations of numerous fractions were observed after electrophoretic separation, undialyzed ragweed pollen extracts were studied with more satisfactory results.

Early experiments with dialyzed extracts in the Tiselius cell showed that a highly skin reactive, major, unpigmented, negatively charged, slowly migrating component (USG) was present in giant ragweed pollen extracts. A similar component (USD) was also present in dwarf ragweed pollen. There were usually also six or more minor pigmented components in both types of solutions. The major unpigmented component of giant ragweed extract was present over the pH range of 3.5 to 7.5 without apparent dissociation. In one experiment, however, the USG fraction showed two peaks at pH 3.5.

The major colorless components are electrophoretically homogeneous. In the ultracentrifuge at 150,000 g they show a single band with the Foucault-Toepler scanning method. As yet, only two fast pigments, FPG and FPD, have been isolated. It is difficult to separate the other minor pigments, because their electric mobilities lie very close together. In physiological salt solution, the colorless components are practically stationary in the electric field. In the presence of less salt, they, as well as the fast pigments, are negatively charged between pH 4.5 and pH 7.5. We have not, as yet, determined the isoelectric point accurately because of the low electric mobilities observed. The USG and USD boundaries moved during ultracentrifugation with almost identical speeds. The sedimentation constants at 20° are about $1.5 \times 10^{-13}\text{cm}^{-1}$. This value is smaller than the smallest sedimentation constant in the table of proteins in Svedberg and Pedersen's "Ultracentrifuge." Ultracentrifugation of crude giant and crude dwarf ragweed solutions showed that the main boundary sediments at about the same rate as the USG and USD components. On direct observation during ultracentrifugation, the pigment above and below the boundaries was equal in intensity of color, but did not separate.

The diffusion constants of the USG and FPG components show that the colorless fraction and the fast pigment diffuse rapidly compared with proteins like serum albumin. The curves also provide evidence that homogeneous systems are being observed. The data thus far obtained at 2°C indicate that the diffusion constant for the USG is $1.7 \pm 0.5 \times 10^{-6}$ cm ^{2}/sec. The diffusion constant for the FPG fraction is slightly smaller. The rapidity of diffusion and the slowness of sedimentation indicate that the major colorless component and the fast pigment have molecular weights which are low when compared with ordinary proteins. Calculations based on these values of the constant resulted in a molecular weight of about 5000 for the colorless components.

The homogeneous USG component has been named trifidin. Experimentally, it produces hay fever and asthma. Similar clinical activity was observed for artefolin (the USD component). It is of interest that the fast pigments are also highly skin reactive. Since the fast moving pigment and the slow moving colorless components are both highly skin reactive, it is tempting to speculate that the colorless component has an immunologically active group which is common to and bound to the pigmented components. The nature of the binding would be of the type not broken by the electric field. This hypothesis of a "master" antigen can be tested by suitable immunological procedures. These experiments are in progress with Dr. G. Shwartzman.

The major colorless components are readily precipitated by phosphotungstic acid and the Millon reagent. In the concentrations which we have employed, they show opalescent or delicate turbidity with the usual protein reagents. The Biuret and Molisch tests are positive.

Have we to do with an ordinary protein in the case of these active materials? It is difficult to say. This much, however, is certain. We have isolated substances of comparatively low molecular weight which have certain protein-like characteristics. Whether it is an ordinary protein or whether it is part of a group of substances related to proteins, which are characteristically to be found in the pollen, allergens may only be discovered by further experimentation. Preliminary data obtained with Dr. Moore indicate that during the electrophoretic analysis of timothy grass extract, an electrophoretically homogeneous, highly skin reactive, colorless component may also be obtained. Similarly, in the timothy pollen extracts, there are at least six pigments and the patterns of timothy closely resemble that of the ragweed extract.

All of our findings may now be assembled. The low value of the molecular weight readily accounts for the ease with which the biologically active materials penetrate the skin when the electric field is applied. It also adds clarity to the fact that persons capable of being sensitized to ragweed are so frequently sensitized. This is so because one of the most important factors in the production of sensitization should be the rapid diffusion of the active allergenic materials, which leak out of the pollen granules and pass through the mucous membranes of the nose and bronchioles into the tissues beneath.

We have not explained, however, why it is that the electric field is so efficacious in forcing the pollen allergens into the skin. You may recall that I have pointed out that the colorless materials move very slowly in the electric field. The pigments move more quickly. But the slow moving trifidin can be introduced in the absence of pigments. To explain this phenomenon, Gorin and I have shown that a hitherto neglected factor (the diffusion forces) may operate in the pores of the skin. Taking these diffusion forces into account simplifies our explanation of the rapidity of movement of the pollen through the pores of the skin, in this case, through the sweat glands. The electric field may electro-osmotically set up liquid bridges in the pores through which appreciable diffusion may be facilitated.

With Dr. Moore and Mrs. Gettner further studies of the physico-chemical nature of other pollen allergens as well as the development of therapeutic technics are planned. On some future occasion I hope to have the pleasure again of addressing this section on the results of this project.

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SECTION OF PSYCHOLOGY

November 17, 1941

Doctor Charlotte Buehler, College of the City of New York, and Clark University, Worcester, Mass.: *Symbolic Action in Children.* (This lecture was illustrated by charts, drawings and opaque projection.)

The idea of this paper is to connect the demonstration of a new test with the discussion of a fundamental problem in child psychology and psychotherapy: namely, the problem of symbolic action in children.

A symbolic action is an action in which the overt behavior, the direct expression, and the manifested purpose stand for another behavior, expression, and purpose than the manifested ones.

The first question which is discussed is: When does, and when does not, the make-believe play or also any other play mean and express something beyond its admitted, overt, direct content? Erik Homburger Erikson's analysis of the case of Mary[1] is taken as a starting point for the discussion of those features in her play which are so age-characteristic that, without further specific evidence, they cannot be assumed to be an individual expression of an individual problem, as compared with other features in her play which are very individual and which, therefore, are much more likely to express and to mean something.

The second question which is raised is: Which items or features of an action are used for symbols? Content and form can both be used for symbolic expression. Enclosures are used, as an example, for the discussion of a formal symbol and the next question is then raised whether one and the same item expresses always the same or quite different ideas. Ernest Schachtel's study on perception of adults[2] seems to show, correspondingly to play observations with children, that enclosures can mean quite opposite things, namely shelter as well as imprisonment. There are still further meanings in children's use of this symbol.

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[1] Erikson, Erik Homburger. Studies in the interpretation of play I. Genetic Psychology Monographs, 140, 22, 557-671.

[2] Schachtel, Ernest. The Dynamic perception and the symbolism of form. Journal of the Biology and Pathology of Interpersonal Relations, 4, 79-96 (1941).

The last question raised in this general discussion is: Whether every human being has the same disposition to express himself symbolically. Again, in agreement with Schachtel's observations with the Rohrschach Test and with observations of Bender and Schilder[1], the hypothesis is formulated that the threshold for the disposition to produce and to perceive symbols in individually a very different one.

The World Test is briefly introduced.[2] This test originated from Margaret Lowenfeld's World Play, the use of which she described some years ago.[3] The test is based on the analysis of 60 worlds of Viennese school children from 4 to 9 years of age; 25 worlds of Minneapolis kindergarten children of 4 and 5 years; 180 worlds of clinical cases of European and American children from 5 to 16 years.

The test and present analysis makes use so far of 7 characteristics of the worlds. These characteristics are called symptoms because they discriminate significantly problem children and retarded children from normal adjusted children. The seven characteristics so far studied are empty worlds with less than 50 elements; worlds in which only 5 types of elements or less are used; worlds without people; worlds with soldiers or other aggressive elements and with aggressive action like killing, accidents, burying; worlds with certain formal characteristics like rows, enclosures, chaotic distribution of elements and two degrees of organization.

Some of the results which were discussed were: (1) The characteristics called symptoms occur significantly more frequently with retarded children than with problem children; with problem children than with normal children (ratio roughly 3:2:1); more frequently with younger than with older children; with girls than with boys. This means that they occur in increasing number with increased weakness or incapacity to cope with life. (2) The symptoms seem to be partly unspecific and partly specific. The unspecific symptoms occur with various types of conditions and problems. The specific symptoms occur in significant correlations with certain types of conditions and problems.

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[1] Bender, Loretta, and Schilder, Paul. Form as a principle in the play of children. Jour. of Genetic Psychology 49, 254-261 (1936).

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[3] Lowenfeld, Margaret. The World Play. Brit. Jour. of Medical Psychology, 18, 65-101 (1939).

Unspecific symptoms are empty worlds which occur also with normal children and which may indicate, in a majority of cases, just lack of imagination and nothing more. Worlds without people and worlds in which fight takes place seem also to occur with almost any type of problem child whose contact with people is disturbed. There are, however, some exceptions to this observation, in that there is a significant negative tetrachoric correlation between hysterical children and worlds without people. That is to say, hysterical children generally do not make worlds without people. There are further positive and negative correlations as regards the number and use of aggressive elements with various types of problem children.

Among the specific symptoms, the enclosures and the chaotic worlds are of outstanding importance. They lead through the study of quite a number of significant correlations to the observation that two main opposite neurotic phenomena can be established. The one is the phenomenon of disintegration and confusion, the other is an opposite extreme, namely, too rigidly keeping together and closing together. One is giving up and losing hold over oneself; the other is protecting and closing off oneself with a certain neurotic strength.[1] These observations in connection with the correlations on which they are based, suggest different techniques in the approach to these two groups of children when they are to be treated.

[1] Saul Rosenzweig considers these observations in good accord with his and David Shakow's findings with schizophrenics. See "Play Technique in Schizophrenia." American Journal of Orthopsychiatry. 7, 31–47 (1937).

SECTION OF ANTHROPOLOGY

NOVEMBER 24, 1941

DOCTOR RALPH LINTON, Columbia University, New York, N. Y.: *Some Recent Developments in Southwestern Archaeology.*

The archaeology of the Southwestern United States has been studied more intensively than that of any other part of North America and a fairly clear picture of the interrelations and developmental sequences of the various cultures present there is beginning to emerge. That early man was contemporary, with a Pleistocene fauna in this region, seems to be proved. However, even the earliest remains indicate the presence of a multiplicity of cultures whose origins and interrelations are still uncertain. These cultures fall into two main groups. One group is characterized by finely chipped stone implements, especially projectile points, among which the Folsom and Yuman types are the best known. Their economy seems to have been based upon big game hunting. The other group shows a striking lack of finely chipped implements, especially in the lower levels, but is characterized by the presence of great numbers of grinding stones. Their economy seems to have been based upon small game hunting and seed gathering. The distribution of these two groups in the Southwest is still imperfectly established but the seed gathering cultures seem to have been most strongly developed in the southern and western parts of the area. The big game hunting complex, on the other hand, seems to have been most strongly developed on the northeastern borders of the area and is linked with the earliest cultures of the high plains. The presence of occasional implements of big game hunting types in sites of the seed gatherers indicates that the two complexes were contemporaneous, while the same Pleistocene fauna is associated with both.

No connections have so far been established between the cultures of the big game hunters and the later agricultural peoples of the region. One of the seed gathering cultures, the Cochise, shows many resemblances to a later agricultural culture, the Mogollon. Since the latter dates from only slightly before the beginning of the Christian era, late survival of the earlier culture is suggested. The similarity of implement types throughout all Cochise horizons is so marked that it seems improbable that this culture has the great age assigned to it by geologists.

The later agricultural cultures of the Southwest can now be reduced to three main groups with a possible fourth. The established groupings are the Hohokam, Mogollon, and Anasazi. The Hohokam centers in southeastern Arizona. Its origins are obscure, but it shows, from the earliest levels, numerous resemblances to the higher cultures to the south, in Mexico. The Mogollon also centers in Arizona, but to the north and east of the Hohokam area. It seems to be the earliest agricultural, pottery-using culture in the Southwest and to have developed out of the local Cochise culture. Mogollon sites are strictly linked with an environment suited to the growing of maize without irrigation or the development of drought-resisting varieties, i.e. well adapted to the beginnings of its cultivation, when it was first diffused from Mexico. In spite of its early beginnings and influence on neighboring cultures, the Mogollon culture never developed great complexity and was eventually absorbed by the expansion of Hohokam and Anasazi. The Anasazi group centers to the north and east of the Mogollon and has been studied more intensively than any of the other groups. It begins with the maize-cultivating but potteryless people of Basket Maker II and culminates in the great communal dwellings of the Chaco and Mesa Verde. There are many indications that it received the idea of pottery-making from the Mogollon.

The possible fourth group of cultures centers on the northeastern borders of the Southwestern area. First recognized in one of its later phases, the Gallina, Columbia University's work of the past summer, has pushed this line back to about 700 A. D. Even then, it possessed full agriculture and pottery. Sites of this culture in the Gobernador region of northern New Mexico are characterized by small settlements consisting of one to at most four or five dwelling complexes. Each complex was presumably occupied by some sort of kin group. It has as its central feature a very large and deep circular pit house with small central fireplace, banquette and ventilator shaft, reminiscent of the kivas of the later Anasazi cultures of this region. Associated with this, on the surface, were rows of small rooms, apparently used as granaries, numerous fireplaces and shades. Some pit houses have associated with them small, shallowly excavated circular structures suggestive of menstrual lodges. The whole site is commonly surrounded by a fence or stockade. All the pit houses excavated had been burned after the interiors had been put in order; metates placed in one part of the house; pottery in another, etc., suggesting some sort of ritual destruction. All burials found were secondary, after the exposure of the body for some time. Most of the small bones were missing, while one skull contained a wasp's nest.

Pottery is of two types: bowls of a painted ware closely related to the contemporary Basket Maker wares and cooking pots and jars of a coarse dark grey ware apparently not related to Basket Maker. The latter ware was always made in baskets about two inches high, the walls of the vessel being carried up above this by coiling. The exterior was frequently rubbed with a bunch of grass or pine needles giving a striated surface. Agriculture is proved by finds of charred maize.

This early phase seems to pass through gradual modifications in house types into the typical Gallina. In this level, the painted ware still shows strong similarities to the contemporary Anasazi wares, but the utility ware ceases to be made in baskets and assumes a form very similar to that of the historic Navajo utility wares.

The painted wares of this group of cultures are clearly dependent upon Anasazi influence throughout their history. The utility pottery, on the other hand, shows strong resemblances to coarse wares, whose distribution extends northward and northeastward into the Plains and Plateau. It is characteristic of these coarse wares that they were employed almost exclusively as cooking pots. This use of pottery exclusively as a food-boiling appliance, bowls, spoons, etc. being made of other materials, is characteristic of the far northern pottery using cultures of both the Old and New Worlds. The rarity and small size of cooking vessels, as distinct from bottles, bowls, etc., in the Basket Maker III culture, suggests that a stimulus toward food-boiling may have come into the Anasazi line from some northern culture, bringing with it a distinctive type of utility pottery. The sharp distinction in surface and paste, which distinguishes Anasazi cooking wares from other wares in simultaneous use, may reflect an original diversity of origin.

It is suggested that the appearance of the Gallina line in the Southwest may be correlated with the intrusion into the area of some northern group, who brought with them, not only a new type of utility ware, but also the deep pit house, a widely distributed circumpolar feature. The sudden appearance of this structure, which has no Basket Maker prototype, and its persistence in later Anasazi cultures as the kiva, may be a result of borrowing.

It is tempting to assume that the people of the Gallina line were the ancestors of the later Navajo, but the only close similarities between the two cultures are those found in the utility pottery. The gap between the latest Gallina and the earliest Navajo sites still remains to be bridged.

SECTION OF PHYSICS AND CHEMISTRY

November 14 and 15, 1941

Conference on *"The Ultracentrifuge."*

The Section of Physics and Chemistry held a Conference on "The Ultracentrifuge", the first of the series this year. Doctor Duncan A. MacInnes, The Rockefeller Institute for Medical Research, New York, N. Y., was in charge of this meeting, as Conference Chairman.

The program consisted of the following papers:

"Introductory Remarks," by Duncan A. MacInnes.

"The Production and Maintenance of High Centrifugal Fields," by J. W. Beams, Rouss Physical Laboratory, University of Virginia.

"Optical Problems of the Ultracentrifuge," by J. W. Williams, University of Wisconsin.

"The Integration of the Differential Equation of the Ultracentrifuge," by W. J. Archibald, National Research Council, Canada.

"The Present Status of Molecular Weights of Proteins," by Alexandre Rothen, The Rockfeller Institute for Medical Research.

"The Effect of Centrifugal Fields on the Electromotive Force of Galvanic Cells," by Duncan A. MacInnes.

NEW MEMBERS

ELECTED NOVEMBER 3, 1941

LIFE MEMBER

Evans, Florence L., Ph.D., Associate Bacteriologist, U. S. Marine Hospital, Carville, La.

ACTIVE MEMBERS

Collins, Robert E. Lee, Ph.D., Associate Professor, Geology and Geography, University of Tennessee, Knoxville, Tenn.

Conroy, Peter J., Ph.D., Professor, Chemistry, College of Pharmacy, Fordham University, New York, N. Y.

Coons, Robert Roy, Ph.D., Head, Natural Sciences, George Pepperdine College, Los Angeles, Calif.

Cooper, Charles L., Ph.D., Professor, Industrial Education, Agricultural and Technical College, Greensboro, N. C.

Cooper, Stewart Rochester, Ph.D., Assistant Professor, Chemistry, Howard University, Washington, D. C.

Corlis, Edwin Fleming, Ph.D., Professor, Biology, St. Francis College, Brooklyn, N. Y.

Craig, Gladys M., M.Sc., Bacteriologist and Protozoologist, Doctor's Hospital, New York, N. Y.

Crouse, Helen V., Zoology, Columbia University, New York, N. Y.

Dante, John Henry, M.S., Paleoichthyology, Zoology, Washington, D. C.

De Leon, Benjamin, M.A., Teacher, Physics, South Side High School, Newark, N. J.

Eddy, Bernice Elaine, Ph.D., Associate Bacteriologist, Division of Biologics, National Institute of Health, Bethesda, Md.

Ellickson, Raymond T., Ph.D., Instructor, Physics, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

Ellis, Emory L., Ph.D., Instructor, Biochemistry, California Institute of Technology, Pasadena, Calif.

Fay, Temple, M.D., Professor, Head, Neurology and Neurosurgery, Temple University, School of Medicine, Philadelphia, Pa.

Felton, Lloyd D., M.D., Sc.D., Senior Surgeon, National Institute of Health, U. S. Public Health Service, Washington, D. C.

Fosbinder, Russel J., Ph.D., Director of Research, Vice-President, The Maltbie Chemical Company, Newark, N. J.

Foxe, Arthur N., M.D., Instructor, Neurology and Psychology, Bellevue Medical College, New York, N. Y.

Francis, Thomas, Jr., M.D., Sc.D., Professor, Chairman, Epidemiology, University of Michigan, School of Public Health, East Hospital, Ann Arbor, Mich.

Gardner, William U., Ph.D., Associate Professor, Anatomy, Yale University, School of Medicine, New Haven, Conn.

Gollomb, Joseph, M.A., Writer, Psychology, New York, N. Y.

Goodfellow, Thomas John, M.D., Surgeon, Biology; Eye, Ear, Nose and Throat Division, Saratoga Hospital, Saratoga Springs, N. Y.

Goorley, John T., Ph.D., Director of Research, Pharmacy and Medicine, Lex Laboratories, Habana, Cuba.

Gordon, William Henry, M.D., Chief of Staff, Internal Medicine, North End Clinic, Detroit Mich.

Gray, Edward LeB., Ph.D., Research Biochemist, Lever Brothers, Wellesley Hills, Mass.

Gruber, Rudolf E., Sc.D., Chemistry, Biology, Vice-President, Merck and Company, Inc., Rahway, N. J.

Gruse, W. A., Ph.D., Industrial Research, Chemistry, Petroleum Technology, Mellon Institute, Pittsburgh, Pa.

Shoemaker, Norman Clifford, M.D., Staff Member, Biology and Chemistry, Doctor's Hospital, New York, N. Y.

Storandt, Kenneth M., M.S., Psychology and Anthropology, Faculty, New York School of Social Work, Columbia University, New York, N. Y.

ASSOCIATE MEMBERS

Beard, Joseph W., M.D., Assistant Professor, Surgery, In Charge, Experimental Surgery, Duke University, Durham, N. C.

Brussel, James Arnold, M.D., Chief Neuropsychiatrist, Station Hospital, Fort Dix, N. J.

Cooper, Frank L., Ph.D., Instructor, Sloane Physics Laboratory, Yale University, New Haven, Conn.

Cruess, William V., Ph.D., Professor, Food Technology, University of California, Berkeley, Calif.

Curtis, James W. A. S., M.S., Assistant Trade Test Officer, U. S. Army, Jefferson Barracks, Kirkwood, Mo.

Davis, Harry A., M.D., Instructor, Surgery, Louisiana State University, School of Medicine, New Orleans, La.

DeFalco, Ralph J., Ph.D., Medical Research Assistant, Sharp and Dohme, Glenolden, Pa.

Dodge, Ruth A., M.S., Teacher, Biology, Johnstown, N. Y.

Eaton, Orson N., Ph.D., Associate, Genetics Investigations, U. S. Department of Agriculture, Hyattsville, Md.

Ells, Victor R., Ph.D., Instructor, Chemistry, University of Missouri, Columbia, Mo.

Golden, I. J. K., M.D., Medicine, Chicago, Ill.

Hall, David G., B.S., Associate Entomologist, U. S. Department of Agriculture, Bureau of Entomology and Plant Quarantine, Washington, D. C.

Hanmer, Hiram Rupert, B.S., Director of Research, Chemistry, American Tobacco Company, Richmond, Va.

Hildreth, H. M., Ph.D., Assistant Professor, Psychology, Syracuse University, Syracuse, N. Y.

Pillemer, L., Ph.D., Senior Instructor, Immunology, Western Reserve University, School of Medicine, Cleveland, Ohio.

TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY[1]

DECEMBER 1, 1941

PROFESSOR ERLING DORF, Princeton University: *Application of Paleobotany to the Cretaceous-Tertiary Boundary Problem.* (This lecture was illustrated by lantern slides.)

Geologic boundaries, like political boundaries, are notoriously troublesome and subject to change. The correct placing of boundaries between rock units involves difficulties with which most geologists are familiar. These rock units are generally assigned to units of the geologic time-table by means of their fossilized organic remains, wherever possible. Important observable changes in the faunas and floras of successive rock units have led to the recognition of the nebulous abstractions known as "time boundaries" between geologic epochs, periods, and eras. Such boundaries, though obviously as non-existent as any observable boundary between Monday and Tuesday, are translated into concrete reality by reference, if possible, to the horizon in the rock succession at which the organic changes are seen to take place.

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[1] No meetings were held in December by the Sections of Psychology and Anthropology.

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To be specific, the boundary between Mesozoic time and Cenozoic time cannot be observed, except in terms of a boundary between rock units containing recognized Mesozoic fossils and recognized Cenozoic fossils.

By universal agreement, fossils of the Cretaceous system have come to be regarded as belonging to Mesozoic time and those of the Tertiary system, to Cenozoic time. The well-recognized and widespread Cretaceous fossils include ammonites and certain groups of pelecypods, dinosaurs and primitive mammals, and primitive dicots and conifers. In the Tertiary system, ammonites and dinosaurs are lacking, as are most of the genera of primitive dicots and conifers. Instead, are found gastropods and different groups of pelecypods, modernized mammals, and modernized dicots and conifers. The placing of the Cretaceous-Tertiary boundary is, therefore, a problem involving agreement among invertebrate paleontologists, vertebrate paleontologists, and paleobotanists as to the more or less exact horizon where the observable change from Cretaceous to Tertiary organisms actually takes place.

In the sedimentary succession of the Atlantic, Gulf, and Pacific coast regions, the Cretaceous-Tertiary boundary has been established between successive marine deposits and, therefore, has been primarily a problem for invertebrate paleontologists. In the western interior, however, the succession includes both marine beds with invertebrate remains, and terrestrial beds with plants and vertebrates. Here, the problem is to place the boundary at a stratigraphic level acceptable alike to the students of invertebrates, vertebrates, and plants. That there has continued to be a problem in connection with the boundary in this region for over 30 years is evidence of an unfortunate lack of agreement among paleontologists which, it is hoped, will not continue much longer.

In approaching a clarification of the paleobotanical aspects of the Cretaceous-Tertiary boundary, one might well first ask, "What's the argument all about?" In 1921, Matthew defined the issue as follows: "A Cretaceous vertebrate fauna was found associated with a Tertiary flora. Vertebrate paleontologists and paleobotanists took opposite sides; the stratigraphic geologists were divided . . . this discrepancy has been maintained and confirmed by all subsequent work."

This disagreement arose chiefly from data and collections obtained from beds referred to the Lance formation in eastern Wyoming, Montana, and the Dakotas. In the same year, 1921, Stanton reported that the marine invertebrates of the upper (Cannonball) member of the Lance were of late Cretaceous age, thus aligning the invertebrate and vertebrate paleontologists together on one side of the fence and the paleobotanists alone on the other. The stratigraphic relations of the various units involved in the argument were, at about this time, generally understood to be as follows:

	Eastern Wyoming	*Montana and the Dakotas*	
Tertiary	Fort Union	Fort Union	Tongue River member Lebo member
Tertiary? Upper Cretaceous?	Lance	Lance	Tullock, Ludlow, or Cannonball member Hell Creek member
Upper Cretaceous (Montana group)	Fox Hills sandstone	Fox Hills sandstone	

Of these units, only the Fox Hills sandstone and the Cannonball member of the Lance had yielded marine invertebrates. The remaining units contained terrestrial plants and vertebrates.

My own connection with the paleobotanical aspects of the Cretaceous-Tertiary boundary problem began in 1934 with the study of the Medicine Bow flora of southern Wyoming and continued in 1936 and 1938 with the flora of the Lance formation at its type locality in eastern Wyoming. My conclusions may be briefly summarized as follows:

(1) A complete analysis of the flora of the Lance formation at its type locality indicates its late Cretaceous age. The flora is definitely not an early Tertiary "Fort Union" flora, as Knowlton had concluded. It is now clear, as Stanton had intimated as early as 1909, that Knowlton's so-called "Lance" flora was in reality a mixed flora, obtained only in part from the true dinosaur-bearing Lance. A large part of the plant collection had actually been obtained from beds which are stratigraphically above the true Lance

and which are neither genetically nor paleontologically related to the true Lance.

(2) Analyses of the Medicine Bow and Lance floras of the type localities show that these late Cretaceous floras correlate with the flora of only the lower (Hell Creek) member of the Lance and do not correlate with the flora of the so-called upper (Tullock-Ludlow) member of the Lance in Montana and the Dakotas. Analyses of the type Tullock and type Ludlow floras indicate their early Tertiary (Paleocene) age. Further investigation of the localities from which Knowlton had obtained his so-called "Lance" flora reveals that nine localities are in the Tullock or Ludlow, and, in addition, ten other localities are in lithologic equivalents of the Tullock or Ludlow, above the true dinosaur-bearing Lance. By including the plants (38 species) from these localities in his "Lance" flora, Knowlton was obviously impressed by the Tertiary "Fort Union" aspect of the flora. Knowlton, apparently, was led to believe that the Tullock and Ludlow beds were justifiably regarded as part of the true Lance formation on the basis of reliable evidence—though certainly not plant evidence. That the evidence was not reliable has been repeatedly pointed out by vertebrate paleontologists, notably Simpson, as early as 1929. Apparently, a single dinosaur horn core, found loose on the surface, was the only paleontological basis for the original inclusion of the Tullock in the Lance. Subsequent search in both the Tullock and Ludlow beds has failed to turn up additional dinosaur remains. Unfortunately, however, no other reliable vertebrate remains have been found in the Tullock or the Ludlow. Their age assignment to the early Tertiary by the vertebrate paleontologists has, therefore, been based upon negative evidence. The positive evidence of the plant remains substantiates such an assignment.

(3) The paleobotanical evidence now places the Mesozoic-Cenozoic boundary between the true dinosaur-bearing Lance formation and the overlying basal unit of the Fort Union group. In Montana, this corresponds to the boundary between the Hell Creek formation (= true dinosaur-bearing Lance) and either the Tullock formation or the Ludlow formation (from west to east),

both of which were originally, and erroneously, regarded as upper Lance. In eastern and central Wyoming, the boundary is placed between the type Lance or its recognized equivalents and the overlying Fort Union "formation." Throughout this region, except near the Montana border, the Tullock-Ludlow equivalents were correctly included, both originally and subsequently, in the Fort Union sediments above the true dinosaur-bearing Lance.

(4) In the Dakotas, the paleobotanical evidence was, until 1940, not in harmony with the invertebrate evidence regarding the position of the Cannonball marine beds in the geologic time-scale. Since the Cannonball beds interfingered westward with the Ludlow formation, which had yielded a Paleocene flora, the Cannonball was naturally regarded as above the Mesozoic-Cenozoic boundary. This stratigraphic-paleobotanic conclusion disagreed with the conclusion of Stanton that the invertebrates of the Cannonball were Cretaceous. The recent description of a Paleocene foraminiferal fauna in the Cannonball by Fox and Ross seems finally to bring into harmony the varying views regarding the age of these and equivalent beds, as follows:

	Eastern Wyoming	*Montana and the Dakotas*	
Tertiary (Paleocene)	Fort Union "formation"	Fort Union group	Tongue River formation Lebo formation Tullock, Ludlow or Cannonball formation
Upper Cretaceous	type Lance formation	Hell Creek formation	

(5) In other parts of the western interior, a comparison of various controversial floras with those of the type Lance, Hell Creek, and Fort Union floras has given rather positive results.

In the Denver basin of Colorado, the floral evidence points definitely to the assignment of the Laramie, the Arapahoe-Lower Denver, and the lower Dawson formations to the late Cretaceous, and of the upper Denver and upper Dawson to the Paleocene. This conclusion has recently been corroborated by the report by Brown and Gazin of the discovery of Paleocene mammals in the upper part of the Denver formation, above a recognizable lithologic break.

In south-central Colorado, the Vermejo and lower Raton floras are now interpreted as of late Cretaceous age and the upper Raton, as Paleocene. The exact position of the Mesozoic-Cenozoic boundary in this section has as yet not been determined. In western Colorado, floras from the beds referred to the "Laramie" formation are regarded as late Cretaceous and those from the so-called "Post-Laramie," as Paleocene. The Mesozoic-Cenozoic boundary is in this region well-marked by a distinct unconformity.

In southern Wyoming, the floral evidence places the Mesozoic-Cenozoic boundary between the Medicine Bow formation and the Ferris formation at a well-defined lithologic break. In southwestern Wyoming, the flora of the Black Buttes "Laramie" is late Cretaceous, whereas that of the overlying "Post-Laramie" is Paleocene. The boundary between these two units is generally marked by an angular unconformity.

These conclusions may be diagrammatically summarized as follows:

	Central and Eastern Colorado		*South-central Colorado*	*Southwestern Wyoming and adjacent Colorado*	
Paleocene (Fort Union)	Upper Denver	Upper Dawson	Upper Raton	Hanna-Ferris	"Post-Laramie"
Late Cretaceous (Lance)	Lower Denver-Arapahoe	Lower Dawson	Lower Raton-Upper Vermejo	Medicine Bow	"Laramie"

The problems in connection with the Mesozoic-Cenozoic boundary are by no means settled. Yet, there seems now to be a nearer approach to agreement between the interpretation of the vertebrate, invertebrate, and plant evidence, as to its position than at any previous time. We still know very little, however, of the reasons why such marked changes in the organisms of both land and sea took place at so nearly the same "moment" in geologic time. There is still much to be done.

SECTION OF BIOLOGY

December 8, 1941

Doctor Albert Claude. The Rockefeller Institute for Medical Research, New York, N. Y.: *Mechanical Separation of the Morphological Constituents of the Cell.* (This lecture was illustrated by lantern slides, microscopic preparations and specimens).

Extensive use of microscopical techniques and of selective staining in the study of cell structure has made it possible to distinguish two main components in the constitution of protoplasm: (a) a fundamental ground substance, capable of reversible gelation; (b) a number of inclusions which may vary by their size, their shape, their chemical composition and their complexity. The most conspicuous of these inclusions is the nucleus, itself characterized by two distinct formations, the chromatin and the nucleolus. Nuclei and mitochondria are present in every active cell and are, apparently, indispensable constituents of protoplasm. Other inclusions appear only in the cytoplasm of certain cell types and represent a specialized function of the cell. Belonging to the latter category are the secretory granules, such as the zymogen granules of the salivary gland and pancreas, and pigment granules, such as the melanin granules present in abundance in amphibian liver. Little is known regarding the chemical composition of the different cell structures, although their existence has been demonstrated repeatedly by the use of a variety of cytological techniques. The reason for the lack of information is that the analysis of the cell constituents had to depend on micro-tests which are not chemically specific. One exception is the nucleal reaction of Feulgen which, under proper conditions, will detect the presence of thymonucleic acid.

It is obvious that no great advance can be made in the study of cell structure unless we succeed in separating the various cell constituents and obtain them in quantities sufficient to permit the application of the usual analytical methods. The fact that units of different or similar chemical constitution may exist independently in the relatively uniform medium of the cell indicates that chemical or physical affinity between these elements is not very great. These conditions would appear to be favorable for the separation of cell constituents by mechanical means. In recent years, a number of particulate elements have been isolated from the cytoplasm of various cells and the results indicate that the method may be extended to many other tissues and to other constituents of the cell (1). The method of separation consists in breaking up the cells, suspending the fragments in a proper solvent, usually neutral water, and fractionating the suspension in a high speed centrifuge (1). By using a period of centrifugation of one hour and a centrifugal force of 18000× gravity, small particles ranging in size between 60 and 300 mμ diameter were obtained. Evidence has been presented to show that particles isolated in this manner may be mitochondria or fragments of mitochondria (1). The chemical consitution of these tissue particles appears to be quite uniform, irrespective of the cells from which they are prepared. Essentially, these tissue particles are complexes made up of phospholipids and nucleoproteins of the ribose type, the lipoid portion accounting for 40 to 50 per cent of the whole particle. The free particles agglutinate rapidly in slightly acid solutions. They are destroyed by acid, alkali, and by heat, and disruption of the particles by these agents is accompanied by release of free nucleic acid in the solution. Particles of this type have been found in practically every tissue, and no tissue has been examined so far which did not contain them. They appear to have a universal distribution and, like mitochondria, to be constant constituents of protoplasm.

Two other types of granules were isolated from the guinea pig liver, in addition to the usual small particle. One of these fractions is made of elements 40 to 60 mμ in diameter, thus definitely smaller than the usual "mitochondria" particles. When freshly prepared, this fraction is pink to bright red in color. In the centrifuge, it forms jelly-like pellets which are completely transparent. The origin and function of these sub-microscopic elements have not been ascertained. The third liver fraction is composed of granules large enough to be seen under the ordinary microscope, their size ranging between 0.5 μ and 3 μ in diameter. These large granules are referred to as "secretory granules." Chemically, the large granules are composed of phospholipids, which account for about 20 to 24 per cent of their mass, the remaining part being represented mainly by a nucleoprotein of the ribose type (1). Thus, the secretory granules, in their general make-up, resemble the small particles. The results of elementary analysis, however, indicate that the two fractions constitute two distinct groups of cytoplasmic elements. The small particles have consistently been found to contain 9 per cent nitrogen and about 1.5 per cent phosphorus. Characteristic values for the secretory granules of the guinea pig liver are about 12 per cent nitrogen and 0.9 per cent phosphorus.

From their size, it is apparent that many of the particles isolated in the centrifuge are beyond the power of resolution of the ordinary microscope and their particulate nature must have escaped detection by the usual cytological techniques. The possibility that these sub-microscopic elements may progressively develop into visible granules is a point of particular interest, especially in view of the mode of origin of mitochondria. That a relation may prove to exist between the different classes of granules, namely, the small particles and the large "secretory granules," is suggested by the fact that, so far, all have been found to be complexes made essentially of phospholipids and nucleoprotein, these two main components occurring in various proportions in the different types of granules. In the present study, the terms, mitochondria and secretory granules, to designate the free elements, are used only tentatively, until a complete inventory of the cytoplasmic contents can be made by means of the centrifugation technique.

By the simple technique of differential centrifugation at high speed, pigment in the form of granules can also be isolated from the cell. The liver of amphibia is rich in a brown pigment which resembles melanin (2). In the present work, the liver of *Amphiuma tridactylum* was used as the source of pigment. The melanin granules were separated from the other components of the cell and washed in neutral water, by submitting them to alternate centrifugation of three minutes at 18000 × gravity, and 1 minute at 1500 × gravity. In the dark field microscope, the purified pigments appears to be composed of slightly elongated granules, very uniform in size, and about 1 μ × 0.5 μ in diameter. From their rate of sedimentation, these melanin granules appear to have an unusually high density. The results of chemical analysis are not yet available. As an extension to this work, it has been found that the guinea pig liver also contains a small proportion of "melanin granules" which, in the purified form, are strikingly similar to the pigment granules isolated from amphibian liver.

Since the time of Miescher, chemists have endeavored to determine the chemical composition of cell nuclei. In order to secure material as free as possible of cytoplasmic contaminents, studies on cell nuclei were usually restricted to the analysis of fish sperm, pus cells, and nucleated blood corpuscles (3). In recent years, the rapid development of the work on heredity and structure of chromosomes has created a renewed interest in the chemical composition of chromatin. Work of this laboratory, in collaboration with Dr. James S. Potter, has shown that chromatin strands could be isolated from the resting nucleus, and obtained in a purified form in sufficient quantity for chemical analysis. Morphologically, the purified material is represented by double strands of chromatin, stained selectively by the Feulgen technique. Upon treatment of the dry chromatin with chloroform, the strands take a beaded appearance, due to the fact that staining with the Feulgen technique is now localized on individual grains distributed along the original chromatin thread. The chromatin strands obtained from the cells of mouse leukemia contain 15.7 per cent nitrogen and 3.65 per cent phosphorus. The latter figure suggests that about 40 per cent of the chromatin thread is nucleic acid. Whether this value corresponds to thymonucleic acid exclusively or includes also some nucleic acid of the ribose type has not been determined.

Summary. In the present work, morphological constituents of protoplasm, both of cytoplasmic and nuclear origin, were isolated by mechanical means. The different components of cytoplasm, which have been examined so far, appear to be complex formations characterized by the presence of nucleoproteins of the ribose type occuring in association with various proportions of phospholipids. The type of organization of the cytoplasmic granules contrasts with the composition of chromatin, which is characterized by the absence, or a very low content, of lipids, and a relatively high content of thymonucleic acid. It may be significant, however, that both cytoplasmic and nuclear structures are built on a framework of nucleoproteins.

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REPORT OF THE ANNUAL MEETING

December 10, 1941

The 124th Annual Meeting of the Academy for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at the Hotel Astor on the evening of Wednesday, December 10.

The Corresponding Secretary reported that there are now 46 Honorary Members and 4 Corresponding Members upon the rolls of the Academy. Three deaths were reported during the past year.

The Recording Secretary reported that the most outstanding fact regarding the Academy this year, is the continuance of its substantial progress, in spite of existing world conditions. It has published more noteworthy papers, held more important conferences, and increased its membership, of well known scientific workers, to a greater degree than in any previous year. In addition, it has added another new and important Section, that of Oceanography and Meteorology.

During 1941, the Academy held 7 Business and 26 Regular Sectional Meetings at which 29 stated papers, all of very high caliber, were presented. Six informal receptions were held under the auspices of the various Sections of the Academy, in honor of prominent speakers.

The Section of Physics and Chemistry held four conferences on special subjects of research under the following titles: "X-Ray Diffraction," "Viscosity," "Immunochemistry," and "The Ultracentrifuge." Another conference was held in the field of astronomy, on "The Fundamental Properties of the Galactic System," under the auspices of the Council.

The meetings of the new Section of Oceanography and Meteorology, now the sixth section in the Academy, will take the form of conferences to be held at suitable times during the year.

The Recording Secretary expressed his appreciation and thanks to the members of the Academy for their generous and continued enthusiastic support of our activities, and especially so, during the past year.

One Honorary Member, 5 Life Members, 10 Sustaining Members, 310 Active Members, 215 Associate Members, and 11 Student Members were added to the rolls. Thus, a total of 552 new members were added during the year.

The present membership of the Academy is 2,012, which includes 1 Patron, 105 Life Members, 150 Sustaining Members, 964 Annual Active Members, 691 Associate Members, 51 Student Members, 46 Honorary Members and 4 Corresponding Members. Of these, 238 are Fellows.

The Editor reported that during the fiscal year of 1941, the Academy has published more than in any other single year in its history. The total number of pages is 1613, which is 473 pages more than in 1940. Of this amount, 897 pages were published in the Annals, 228 in the Transactions, and 488 in the Scientific Survey of Porto Rico and the Virgin Islands. In addition to this, there is one paper now in galley proof which will appear as Volumn 42, Article 4 of the Annals. This paper will be 130 pages in length, and will complete the volume, bringing the total number of pages published, to 1743.

A list of these publications is as follows:

ANNALS

Volume XL, Article 5 - "Dielectrics," by William O. Baker, J. D. Ferry, Raymond M. Fuoss, Paul M. Gross, Marcus E. Hobbs, John G. Kirkwood, S. O. Morgan, Hans Mueller, J. L. Oncley, Herbert A. Pohl, J. Shack, Charles P. Smyth, J. H. Van Vleck. Pages 289 482. Published, December 31, 1940.

This completes Volume XL.

Volume XLI, Article 1—"The Internal Constitution of the Stars," by Zdenek Kopal, Robert E. Marshak, Henry Norris Russell, Harlow Shapley, Jaakko Tuominen. Pages 1–76. Published, March 18, 1941.

Volume XLI, Article 2- "Crystalline Protein Molecules," by Edwin J. Cohn, I. Fankuchen, J. L. Oncley, H. B. Vickery, and B. E. Warren. Pages 77–168. Published, May 23, 1941.

Volume XLI, Article 3—"The Primary Process in Photochemistry," by W. Albert Noyes, Jr., E. W. R. Steacie, Hugh S. Taylor, Edward Teller, and W. West. Pages 169–240. Published, June 30, 1941.

Volume XLI, Article 4—"The Amphoteric Properties of Proteins," by R. Keith Cannan, A. Kilbrick, John G. Kirkwood, L. G. Longsworth, A. H. Palmer, and Jacinto Steinhardt. Pages 241–328. Published, July 31, 1941.

Volume XLI, Article 5—"Sensory Cues used by Rattlesnakes in their Recognition of Ophidian Enemies," by Charles M. Bogert. Pages 329–344. Published, August 4, 1941.

This completes Volume XLI.

Volume XLII, Article 1—"A Survey of the Literature on Artistic Behavior in the Abnormal: II. Approaches and Interrelationships," by Anne Anastasi and John P. Foley, Jr. Pages 1–112. Published, August 11, 1941.

Volume XLII, Article 2—"The Fundamental Properties of the Galactic System," by B. J. Bok, Dirk Brouwer, W. J. Luyten, D. A. MacRae, H. R. Morgan, Jan Schilt, Frederick H. Seares, Peter van de Kemp, and Emma T. R. Williams. Pages 113–272. Published, October 15, 1941.

Volume XLII, Article 3—"Studies on the Origin and Early Evolution of Paired Fins and Limbs," by William K. Gregory and Henry C. Raven. Pages 273–360. Published, November 15, 1941.

Volume XLII, Article 4—"Intelligence in Mental Disorders," by Anne Roe and David Shakow. Pages 361–490. *In galley proof.*

THE SCIENTIFIC SURVEY OF PORTO RICO AND THE VIRGIN ISLANDS

Volume III, Part 4—"The Tertiary Foraminifera of Porto Rico," by J. J. Galloway and Caroline E. Heminway. Pages 273–492. Published April 21, 1941.

Volume III, Part 4—Title page and contents.

Volume XVI, Part 3—"Bryozoa of Porto Rico with a Resumé of the West Indian Bryozoan Fauna," by Raymond C. Osburn. Pages 319–488. Published, December 31, 1940.

Volume XVIII, Part 2—"A Large Archaeological Site at Capa, Utuado, with Notes on other Porto Rico Sites Visited in 1914–1915," by J. Alden Mason. Appendix. An Analysis of the Artifacts of the 1914–1915 Porto Rican Survey," by Irving Rouse. Pages 207–304. Published, March 10, 1941.

TRANSACTIONS

Transactions, Series II, Volume 3, together with the title page and table of contents, constituting a 228 page volume have been printed and distributed.

The Librarian reported that 19,657 separate publications were distributed to the members of the Academy, authors, exchanges and the sales list, classified as follows: *Annals*, 13,896; *Scientific Survey of Porto Rico and the Virgin Islands*, 1,365; *Special Publications*, Volume I, "Climate and Evolution," 48; and *Transactions*, Series II, Volume 3, complete volumes, 2,128 and 4,348 single copies.

The Library of the Academy has received 1,406 separate publications, from its exchange institutions.

The Treasurer reported that the activities of the Academy included within the fiscal year ending November 30, 1941, proved gratifying both from the point of view of increased income and administrative achievement.

A total income from all sources of $25,336.13 was received for the year as compared with $21,993.47 received during the previous fiscal year, thus indicating an increase of 15.2%. These larger receipts were substantially due to a record growth in membership and an unusual increase in the sales of publications. Three thousand dollars was received from Puerto Rico, which, with the balance of $676.61 still remaining from the previous year, placed to our credit $3,676.61, as available for financing the current publications of The Scientific Survey of Porto Rico and the Virgin Islands.

A noteworthy increase in publications was made possible in the appropriation of sums totalling $11,583.69, as compared with $7,648.95 for the previous year.

The expenses of the important meetings and conferences, held during the year, totaled $1,068.99.

The record of membership expansion was another important factor which added to the increased income. The amount received from this source was used for administrative purposes; the printing and distribution of our publications, mainly Transactions; and $1,019.00 of it was added to the investment funds of the Academy.

During the year, the investments of the Academy were carefully analyzed, some of the holdings sold, and other securities purchased by the Treasurer, acting in conjunction with the Finance Committee and the Council. These changes were of advantage to the Academy, increasing the book value of its property from $90,136.84 as of November 30, 1940, to $92,490.49 as of November 30, 1941. Of this amount, $76,783.58 is in stocks and bonds. The income derived from all Academy investments amounted to $4,231.86, thus giving a yield of 4.2%, as compared with 4.1% earned during the year which ended November 30th, 1940.

As usual, the books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1941, and audited by the Finance Committee, as provided by the Constitution and By-Laws. The property of the Academy was verified and the Treasurer's Report examined and attested by the Finance Committee as of December 9, 1941.

The A. Cressy Morrison Prizes of $200.00 each for the two most acceptable in any field of natural science, within the scope of the Academy and its Affiliated Societies were awarded to the following papers, entitled:

"Thermodynamic Properties of Solutions of Long-Chain Compounds," by Maurice L. Huggins, Eastman Kodak Company, Rochester, New York, and, "The Vernay Collection of Cretaceous (Albian) Ammonites from Angola," by Otto H. Haas, The American Museum of Natural History, New York City.

Honorable Mention was given to the paper entitled, "Studies on Complement," by Enrique E. Ecker and L. Pillemer, Institute of Pathology, Western Reserve University, Cleveland, Ohio.

The George Frederick Kunz Prize of $200.00 for the best paper offered in the fields of Geology and Mineralogy was awarded to the paper entitled:[1]

"Development of the Northern Allegheny Synclinorium and Adjoining Regions," by G. Marshall Kay, Columbia University, New York City.

The Committee takes great pleasure in congratulating the authors of these papers upon the excellent treatment of their subjects and the fundamental importance of their results.

It seems appropriate, at this time, to call to the attention of the Academy members that, although Mr. Morrison did not offer a prize in the field of astronomy this year, he did contribute the sum of $500.00 toward defraying the publications costs of the paper entitled, "The Fundamental Properties of the Galactic System," which was published in the Annals and consists of the papers presented by the astronomers at their conference held under the auspices of the Academy on May 2 and 3, 1941.

The Committee is pleased to announce that Mr. Morrison has generously renewed the offer of two prizes, each of $200.00, for this coming year, the awards to be made at the next Annual Meeting, which will be held in December, 1942. These prizes, again, are offered for the two most acceptable papers in natural science within the scope of the Sections of the Academy and its Affiliated Societies. The precise terms of the competition will be published in a later issue of the *Transactions* and will be distributed to all members of the Academy and Affiliated Societies.

The following members were elected to Fellowship:

Harold A. Abramson, M.D.
George W. Bachman, Ph.D.
Donald Belcher, Ph.D.
Max Bergmann, Ph.D.
Kenneth Clark Blanchard, Ph.D.
Charles M. Bogert, M.A.
Walter H. Bucher, Ph.D.
Charles Lalor Burdick, Ph.D.
John T. Edsall, M.D.
William Etkin, Ph.D.
Frank Fremont-Smith, M.D.
William J. Hamilton, Jr., Ph.D.
Margaret Mead, Ph.D.
Karl F. Meyer, Ph.D., M.D.
Walter R. Miles, Ph.D.
Melvin Mooney, Ph.D.
William Moore, Ph.D.
Henry W. Nisson, Ph.D.
John B. Reeside, Ph.D.
Curt P. Richter, Ph.D.

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[1] This prize was offered in accordance with the terms of a bequest of the late George Frederick Kunz and is part of the accumulated annual income from the sum of $1,000.

Edward C. Rosenow, M.D., LL.D., D.Sc.
Alexandre Rothen, Chem.E., D.Sc.
Walter I. Slichter, E.E.
Athelstan F. Spilhaus, S.M.
Horace van Norman Hilberry, A.B.
Frank L. Horsfall, Jr., A.M.
Cecil V. King, Ph.D.
Lewis G. Longsworth, Ph.D.
Daniel Ludwig, Ph.D.
John J. Lynch, A.M.
Douglas A. MacFayden, M.D.
Wilbur G. Valentine, Ph.D.
John Christian Warner, Ph.D.
John A. Wheeler, Ph.D.
Charles H. Willey, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists:[1]

Peter J. W. Debye, Ph.D., Sc.D., Physicist, Head, Department of Chemistry, Cornell University, Ithaca, N. Y.

Karl Landsteiner, M.D., Sc.D., Pathologist, Immunologist, Member, Rockefeller Institute for Medical Research, New York, N. Y.

The following officers were elected:

For President
Horace W. Stunkard

For Vice-Presidents
Walter H. Bucher
Dorothea McCarthy
Theodore Shedlovsky
Charles M. Breder, Jr.
Margaret Mead
Athelstan F. Spilhaus

For Recording Secretary
Duncan A. MacInnes

For Corresponding Secretary
W. Reid Blair

For Treasurer
Marvin D. Thorn

For Librarian
Barnum Brown

For Editor
Wilbur G. Valentine

For Councilors (1942-1945)
Erich M. Schlaikjer
C. Stuart Gager
Victor K. La Mer

For Councilor (1942-1944)
Charles C. Mook

For Councilor (1942-1943)
H. Herbert Johnson

For Finance Committee
Herbert F. Schwarz, Chairman
Wayne M. Faunce
Harden F. Taylor

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[1] The class of Honorary Life Membership of The New York Academy of Sciences is limited to fifty.

After the Business Meeting, the following program was presented:

Address by the Retiring President

"The Accident of Size"

by

PRESIDENT ROY WALDO MINER

"Interaction Between the Hydrosphere and the Atmosphere"[1]

by

C. O'D. ISELIN

These papers, and also abstracts of those awarded the Academy Prizes, are included in this issue of the TRANSACTIONS.

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[1] Due to unavoidable circumstances, Mr. Iselin was unable to read his paper in person. At his request, the paper was read by Mr. Athelstan F. Spilhaus.

THE NEW YORK ACADEMY OF SCIENCES

Address of the Retiring President* given at Annual Dinner and Meeting

December 10, 1941

Doctor Roy Waldo Miner, The American Museum of Natural History: *The Accident of Size.* (This address was illustrated by lantern slides and motion pictures).

It is difficult for the average person, in everyday life, to realize how completely he is limited by the range of his imperfect senses in judging of the importance of his environment, and how prone he is to overlook vital entities of its composition merely because of their small size in comparison with himself.

A cluster of common mussels, casual oyster or dead scallop shells, picked up in a tidal stream, fragments of rock-weed or bits of eel-grass, are seen to be covered with hard, spreading, or tubular encrustations of limy consistency or encased in jelly-like layers of variegated pattern. Here and there, tassel-like tufts or branching growths adhere to the stems of the sea-weed. All, however, are so inconspicuous and appear so casual in character, that the stroller by the sea-side passes them over, or, crushing them between his fingers, tosses them aside as of no importance.

Yet, if a few such clusters be placed in a dish of sea-water beneath a microscope, new worlds of life leap into view. The coiled limy tube-like entanglements on the outer surface of the dead scallop-shell resolve themselves into dwellings of gorgeously colored tube-building worms (*Hydroides dianthus*), so delicately flower-like in appearance that they are otherwise known as "sea-carnations." Their heads, protruding from the circular tube-openings, are surrounded with finely branched plumes banded with purple, rose, brown, gray and blue. At intervals, along the branches, gleam the lenses of tiny eyes, having light-sensitiveness sufficiently keen, so that a shadow passing above them causes the worm to contract quickly into its tube.

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*Dr. Miner served as President of the Academy during the years, 1940 and 1941.

Here and there, protrude the mosaic-like margins of the tubes of the sand worm (*Sabellaria communis*), built of carefully selected and matched sand-grains, from which extend the heads of the worms themselves, crowned with circlets of close-set golden spines which move independently, like piano-keys, as they palm food-fragments successively until they drop into the gaping mouth below, with its sensitive tongue sorting over the morsels as they fall.

Blanketing the tube-clusters of these marine worms, and indeed almost the entire surface of the scallop-shell, are irregular brick-red encrustations, which, upon focussing the lens upon them, suddenly display themselves as huge cities of still smaller organisms, dwelling in thousands of closely crowded vase-shaped shells, from the circular openings of which protrude minute golden-yellow lily-like heads, each with a central mouth and translucent stalk-like body. These are a species of moss-animal (*Schizoporella unicornis*). Standing between their close-set shells are erect cylindrical organisms ornamented by series of projecting arms, each terminated by a spherical battery of sting-cells.

From the sides of these delicate creatures, bud saucer-shaped projections, which finally develop radiating tantacles, separate from the parent, and swim away as minute hydroid jellyfish. These are colonies of the hydroid, *Zanclea agassizii.* The story is not ended here, for, by looking closely, one may see, scattered between the hydroids, scores of small, brown, bottle-shaped structures, belonging to the still more minute swallow-tail protozoan (*Folliculina hirundo*), the one-celled translucent inhabitant extending a forked, Y-shaped summit out of the bottle top, equipped with tracts of vibrating cilia, which capture and carry food-particles to the tiny round mouth-opening at the base of the fork.

Now, leaving the scallop-shell with its multitudinous population and picking up a pebble and the bit of rock-weed, we find these, too, are covered with other species of bryozoa. The eel-grass, as well, harbors colony after colony of the ascidian, *Botryllus* *gouldii*. These clothe it with a jelly-like substance, in which radiating communities form star-like patterns. Each ray is a separate individual having a terminal incurrent opening, through which water is drawn in with its contained microscopic food, to be passed through the body and cast out from its excurrent opening, grouped with the others at the colony's center. As it èmerges, it bears with it swarms of the ascidian's tadpole-like larvae. These swim about actively for a while, by means of a tail stiffened by a notochord. They also possess a brain, dorsal nerve-cord, gill-slits, ventral blood-vessel and an eye-spot, indicating a relationship to the base of the chordate stem. Soon, they settle down to an inactive life, become attached by anterior cement glands to the sub-stratum, acquire a swelled head, lose their nerve and their "back-bone," thus undergoing a metamorphosis, during which their gastronomic and sexual features become highly developed and, thus, they decidedly fall backward in the scale of life, providing an analogy not altogether uncharacteristic of certain members of the human species!

Thus, the apparently dead and insignificant crusts on marine shells and seaweed teem with abundant life in complicated and interwoven associations.

Likewise, let us now visit an inland fresh-water lily-pond, lying quiet among the hills of Maine. From its margin, we gather a few of the delicate submerged water-plants and partially fill a jar with some of the pond-water in which they were growing. We bring it back to the laboratory, and, after allowing it to stand in the window for a few hours, take a little of the water, together with a few strands of the water-plants, and place them in a watch-glass under the microscope. Our first glance shows the water filled with life—protozoa, swimming and darting in all directions. *Stentor*, especially, is particularly abundant, propelling its green oval-shaped body actively, by means of its serried membranelles, or, showing off its protean character by attaching itself at one end, transforming its oval into an elongate, slender-stemmed trumpet; while its membranelles, previously used for propulsion, have now become a suction apparatus, creating a whirlpool which captures vast quantities of smaller organisms, engulfing them wholesale in its voracious maw. Rotifers are particularly conspicuous. These interesting creatures were the first animals seen through a microscope. When, about the middle of the Seventeenth Century, Leeuwenhoek first turned his newly invented lens on a drop of pond-water, he beheld myriads of minute living creatures, the existence of which had never been suspected. The rotifers at once attracted his attention because of their comparatively large size and abundance. His attention was particularly attracted to a structure crowning their heads, which he mistook for a wheel, rotating about a central axis. Hence, he called them rotifers (wheel-bearers).

Later, with improved lenses, he found that the "wheel" was a circlet of cilia, which, beating the water in rapid succession, gave the visual illusion of rotation.

These creatures occur in fresh water in great abundance and variety, while a smaller number of species are marine. Their habits also are equally diverse. The circlet of cilia (corona), crowning the head region, surrounds the mouth and, by its successive waves of vibration, like a propeller, draws the rotifer forward. The mouth, situated at the base of the funnel, receives the smaller organisms, swept in by the vortex thus created. At times, a rotifer will anchor itself by the pair of toes at the posterior end of the body, when the power of the whirl-pool becomes intensified, the intake of food being increased correspondingly. *Cyrtonia tuba* is such a rotifer, having a circular corona, while *Rotaria macrura* has two circlets side by side. When this species becomes stationary, it will often draw in its corona, enclosing it in a tube-like fold, and will then crawl up the stem of a water-plant, by attaching its head, and drawing up its posterior end like a "measuring worm." *Notommata copeus*, when swimming, expands its corona to form three projections, one on either side, resembling ear-lappets, and one projecting downward in a median position, something like an elephant's trunk. Its feeding habits are peculiar. It settles down on a filamentary strand of the common pond-scum (*Spirogyra*), draws in its "ear-lappets," extends its throat-jaws, (an apparatus characteristic of rotifers, with many modifications of diagnostic value), bores a neat hole in one of the cylindircal *Spirogyra* cells, and proceeds to pump out the endochrome coiled spirally therein. Then, it crawls up the filament to the next cell and repeats the process, continuing until the entire filament is empty. On the other hand, *Dicranophorus forcipatus* is carnivorous, springing upon its prey, and extending its forcipate jaws to capture it and tear it to pieces.

Water-plants, often associated with rotifers, include not only *Spirogyra*, but also the water thyme (*Anacharis*), and the bladderwort (*Utricularia*). The latter, especially, plays an important part in fresh-water microscopic life and is, at times, crucial for rotifers and other minute creatures of the pond. It is a submerged plant with zigzag stem, having leaves modified to form flattened disk-shaped bladders with an ingenious trap-door at the outer corner, surrounded by spreading branching spines. The stem of *Utricularia* is covered with growths of unicellular algae, diatoms, and desmids, forming a perfect pasture-ground, on which rotifers love to feed. While so engaged, they often become entangled among the spines, and, sooner or later, come in contact with the trap-door of the bladder. A crevice quickly opens with the slightest pressure, and they slip through and are trapped. Soon, they die, suffocated, and gradually macerate. There is no digestive ferment, but the fluid products of decay are absorbed by the plant cells, which is, therefore, carnivorous.

Many rotifer species, such as *Floscularia ringens*, are sessile, forming tubes of mucus or other materials. The species mentioned spins spherical bricks, which it neatly builds into a trumpet-shaped tube. Other rotifers, like *Octotrocha speciosa*, are colonial, forming a spherical jelly-like house, in which the individuals live, radiating out from the center, where they are attached by their tapering toes, and extend their pansy-like heads at intervals from the surface. The members of the *Octotrocha* colony are giants among rotifers, each individual approximating a millimeter in length. These are but a few of the myriad forms that make up the microscopic associations occuring in fresh water ponds.[1]

The complex worlds of life, contained within such minute areas as encrustations on chance shells and sea weeds, and in a few drops of pond water, could easily be blotted out of existence between thumb and fore-finger. Yet they contain myriads of individuals of infinite variety in crowded array, organized to form interlocking environmental associations. They are born, grow, feed, reproduce and die, meanwhile preying upon each other or avoiding their enemies. All higher animals, including ourselves, without regard to size or so-called position in the scale of evolution, can do no more. Their living substance, protoplasm, is identical with our own. Size is merely an accident. Vital significance does not depend upon it as important, even in the general struggle for existence. A microscopic protozoon, *Trypanosoma gambiense*, swimming in the plasma of our blood, infects us with the dread disease, sleeping sickness. Even ordinary street dust is charged with millions of bacteria, both benign and virulent, and these are drawn into our lungs with every breath. It has been calculated that, a pin's weight of street dust, about one decigram, contains, on the average, 4,000,000 bacteria.

We rate highly the physical courage of the big game hunter who faces a charging bull-elephant in the African jungle. Yet, all the elephants that ever existed have not created as much havoc in the human race as one epidemic of malaria in India. We view with amazement the achievements of mankind in building towering sky-scrapers like the Empire State Building. But if we fly in an air-plane high above the city, the huge structures dwindle into insignificance. A Niagara impresses us with its grandeur; the high peaks of the Rockies inspire us with awe; while the Grand Canyon of the Colorado overwhelms us with the magnificence of its excavation, a mile in depth, and five miles from side to side. Its thousands of sedimentary strata, laid down through the ages, were carved out by water erosion during additional eons of time. Yet, if we could, by some magic, ascend so far above the earth's surface that the whole of the North American continent would lie at once within our vision, Niagara would disappear like the merest trickle; the Rockies become trifling excrescences; and the Grand Canyon, an irregular crackle on the continent.

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[1] The speaker, in cooperation with Mr. Frank J. Myers, a noted authority on rotifers, made a survey of a cubic half-inch of pond-bottom, and directed the construction of a group in the American Museum of Natural History, representing this area enlarged 100 diameters, or cubically, 1,000,000 times.

Before lenses were discovered, mankind lived in a thin, comparatively two-dimensional world, looking sidewise about him at creatures and objects within the limits of his short and imperfect vision. But, with the invention of the telescope and microscope, glass windows were given to man, through which he now gazes into the infinity of the microcosm, on the one hand; and with ultramicroscopic powers, even apprehends rotating systems of the protons and electrons of atomic universes. On the other hand, applying his otherwise weak vision to the telescope, he becomes gifted with visual powers that enable him to penetrate through the starry depths of space, light-year after light-year, universe after universe, into the boundless infinity of the macrocosm. Yet, who can say that these huge distances are not, relatively, of the same proportions as those separating the electronic systems in the ultramicroscopic atom? Size is only relative and the merest accident. Importance lies in energy and, especially, in that organized as protoplasmic life, whether it functions in the single cell of the protozoon or in the complicated centers and tracts of the human brain.

THE NEW YORK ACADEMY OF SCIENCES

Address given at the Annual Dinner and Meeting

December 10, 1941

MR. C. O'D. ISELIN, Director, Woods Hole Oceanographic Institute: *Interaction Between the Hydrosphere and the Atmosphere.** (This address was illustrated by lantern slides).

The interaction between the sea and the atmosphere is no longer a subject which can be dealt with briefly. It will be the subject for discussion at the first meeting of the new Section of Oceanography and Meteorology in March. No doubt, it will also be the chief topic at many subsequent meetings, for of recent years our understanding of the heat exchange and of the momentum transfer between the hydrosphere and the atmosphere has been greatly increased. Thus, to cover any of the basic ideas at all thoroughly, tonight, I must pick and choose, and naturally I will try to emphasize the more recent developments.

At the sea surface, oceanography and meteorology meet. From one standpoint, the force which the winds exert on the ocean surface causes the great ocean currents. From the other point of view, this force is retarding the free flow of the winds. While the winds provide most of the energy, it is the ocean, through its great heat capacity, which modifies the climate and transports some of the heat from low to high latitudes. Furthermore, in the vertical sense, the boundary between the two fields of science is not at all sharp. Meteorologists are concerned with a relatively thick layer of the ocean, while oceanographers must take into consideration the physics of a still deeper air column above the sea surface.

From the standpoint of techniques and instrumentation, the two fields of science are also closely allied. Physical oceanographers and meteorologists should have the same sort of mathematical training. In a number of cases, the same investigator is carrying out work in both fields at the same time. In fact, one might almost say that the main distinction, nowadays, between physical oceanographers and meteorologists, is that the latter group is more inclined to sea sickness.

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*Contribution No. 312, from the Woods Hole Oceanographic Institution.

The classical theory of wind-produced currents in the ocean was developed by Ekman (1928) more than 20 years ago. He showed by very direct and satisfactory mathematical analysis that, in a somewhat idealized ocean in the northern hemisphere, a steady wind should, in a few hours, cause the surface layer to move considerably to the right of the wind direction. At the sea surface, this movement would be 45° to the right of the wind and, at increasing depths, the speed would decrease, but the direction of motion would swing more and more to the right until, at a depth of roughly 100 meters, a weak flow should actually move in a direction opposite to that of the wind. This is the so-called Ekman spiral, the net transport of which is in a direction 90° to the right of the wind in the northern hemisphere, to the left of the wind in the southern hemisphere.

These results, which are due to the earth's rotation, also depend on certain assumptions regarding viscosity and on the existence, near the surface, of a sufficiently deep layer free from vertical temperature gradients. In non-mathematical language, the flow consists of many thin layers of water, each one being dragged along by friction with the layer just above, and being retarded by friction with the layer just below. No systematic vertical motions are involved, and, for the present purposes, this is the most important point to note.

The net transport of the surface layer, which in this way is directly influenced by the wind, will lead to a piling up of the waters on the right hand side of a steady wind and this will set up a gradient current at mid-depths, flowing in the same direction as the wind. Near the bottom, friction will again produce a spiral, but this time to the left of the wind direction. The gradient current at mid-depths will also reduce the angle which the surface water takes. So that once a steady state has been reached, the flow at the surface will be 20°-30° to the right of the wind direction, depending on the strength of the gradient current. This, in brief, is the theory of ocean currents arrived at by Ekman. His analysis also applies to air currents and the meteorological aspects of his work are much better known than the oceanographic results. Ekman's mathematics is so convincing and his conclusions so logical that, for many years, oceanographers have not been much worried by the fact that nobody was able to show in a convincing manner that the surface wind current consistently diverged markedly to the right of the wind direction.

A few years ago, further elaboration on Ekman's theory by Rossby and others showed, on good theoretical grounds, that the coefficient of friction between the air and the water was, perhaps, not strictly proportional to the wind velocity, but that there should exist one or more critical wind velocities, at each of which the wind-produced current would considerably increase in strength. Provided one of these critical points came within the normal range of variation in the strength of the trade winds, these studies supplied a mechanism whereby ocean currents might fluctuate quite widely in strength over long periods, while the general wind circulation remained relatively steady.

Such, very briefly, is one modern theory of wind-produced currents. Furthermore, during the last year or two, some most interesting observations have been made and these may require a considerable overhauling of Ekman's results.

In the first place, Langmuir (1938), observing the effect of wind on the surface of Lake George, reported alternating bands of convergence and divergence extending down wind, so that the surface motion was broken up into cells. He also called attention to the lines of gulf weed which are frequently seen in the North Atlantic and which never before had been satisfactorily explained. When lines of gulf weed form, they are always parallel to the wind direction.

About the same time, a study of herring gull flight by Woodcock (1940) pointed to a similar pattern of motion in the lower layer of the atmosphere, whenever the air temperatures were lower than the water temperatures. The cells in the air, the streaks of convergence of which are used by the gulls for soaring, are clearly caused by convection, that is heating from below. Moreover, at low wind velocities, Woodcock showed that only chimney-like updrafts are present, while with winds of from 16 to 28 mph. alternating bands of convergence and divergence obtain. In winds stronger than 28 mph., the gulls are not able to soar at all, no matter how much colder the air is than the water.

Woodcock (1941) then turned his attention to the cells in the water which so nicely account for the streaks of gulf weed. He found that, under most conditions, evaporation from the sea surface is producing a very thin layer of surface water which is colder and therefore heavier than the water just beneath. This cool layer can be seen to drain downward in the lines of convergence. Thus, the motion in the water seems to be also caused by convection, but this point is not yet quite clear, for cells have been observed under conditions where evaporation could not be taking place. Often very small cells can be seen at the surface and these converge into larger cells. When surface cooling is active, the maximum size of the cells is only limited by the thickness of the almost isothermal surface layer.

All of this provides an explanation for many simple phenomena. It explains, for example, why the herring gulls are only observed far off shore during the autumn months. Being lazy creatures and disliking to have to flap their wings more than is absolutely necessary, they keep close to the shore except when the convectional updrafts are well developed. It is now clear why a dry, cool wind kicks up more of a sea than a damp, warm wind of equal strength. A dry, cool wind causes instability and vertical motion in the surface layer and thus the energy of the wind is transferred downward, so that, in effect, the friction between the wind and the water is increased. On the other hand, damp, warm winds slip easily over the sea surface and laminar-like flow occurs in both media. For the same reasons, it is now clear why, in a fog, capillary waves usually do not occur. In a fog, condensation, the opposite from evaporation, is taking place and the surface film of water has very great stability.

The more general oceanographic significance of these ideas has only very recently been considered. In the first place, wherever the winds are off the land they are relatively dry and, in winter, also much colder than the water. In these areas, a relatively deep convectional layer will be formed and the ocean will gain from the winds much more kinetic energy than in regions where the air is nearly saturated with water vapor. Wherever evaporation occurs, and it occurs over a surprisingly large part of the ocean, vertical movements will be present in the surface layer which will cause a modification of Ekman's spiral. In such areas, the flow will be down wind and not markedly at an angle to the right. It is perhaps for this reason that it has not been easy to demonstrate the sort of flow demanded by Ekman's analysis. Finally, I might point out that if the new ideas are substantiated, the strength of the major ocean currents is not only dependent on the strength of the winds, but also on their humidity and temperature, as well.

One reason why oceanographers have been rather slow in gaining a clear picture of what is going on in the surface layer, is the difficulty of making temperature observations at short distances below the surface. In the first place, the ship is usually rolling, so that a thermometer can not be held at a precise distance below the surface. Second, if the ship is stopped, she is usually drifting sideways and thus destroying the very thermal structure which one might hope to observe in the undisturbed surface waters. These difficulties have recently been overcome by the development of an instrument called the bathythermograph. It consists of an extremely responsive thermal element and a pressure element. When allowed to fall freely through the water, it gives a record of temperature against pressure, that is, depth. It can be worked from ships moving at 10 knots or more and at depths down to 150 meters. Since it can be lowered into the water at 10 feet or more from the ship's side, it records the vertical temperature gradients in undisturbed water and, since each lowering only requires 3 or 4 minutes, the observations can be spaced much more closely in the horizontal direction than is practical with conventional, deep sea, mercurial thermometers. The bathythermograph is giving oceanographers an entirely new conception of the thermal structure in the surface layer. The analogous instrument in meteorology is the balloon sonde, although oceanography will always suffer from the great practical difficulties in obtaining synchronous data from a network of stations.

The bathythermograph not only has confirmed the existence of a nearly isothermal convectional layer at the surface and has shown how the thickness of this layer varies from season to season and from place to place, but it has also shown in satisfactory detail the contacts between contrasting water masses.

On the conventional oceanographic diagram, quite wide interpolations between the observations, both vertically and horizontally, are necessary. For example, the cross current distribution of temperature for such a current as the Gulf Stream is usually worked out from stations spaced 15 to 20 miles apart, at each of which temperature is observed at 100 meter depth intervals, except near the surface, where a 50 meter and a 25 meter reading are also usually obtained.

When the bathythermograph is used with lowerings made at 2 to 3 mile intervals, very much more detail is often disclosed. Near the edges of a current, small horizontal eddies (Spilhaus 1940) with a diameter of 5 or 6 miles are often found. These cause vertical displacements of the isotherms, so that the thermal structure in the surface layer in such regions is a great deal more complicated than could ever be known from thermometer observations.

Near the edge of the continental shelf, the relatively fresh coastal water usually overlies the warmer and more saline off shore water. Because of the relatively wide spacing of thermometer observations in the past, such contacts between contrasting water masses have usually been shown as being quite smooth. But the bathythermograph records call for a complicated interleaving of the water layers both vertically and horizontally.

The slowness whereby turbulence is able to blend water masses having contrasting temperatures and salinities, has, of course, been long recognized. It can be seen off the mouth of any river. Moreover, at the surface, the edge of such a current as the Gulf Stream is relatively sharp and this has supported the idea that this current resembles a river of warm water flowing through colder seas. However, we have already seen that the Gulf Stream, in reality, occupies the transition zone between cold and warm water masses, so that, at any given depth, except near the surface, its temperature is intermediate. Only at the surface, is the water near the axis of the current slightly warmer than in the Sargasso Sea. Nevertheless, this thin surface film of northward flowing, warm water is a tremendous transporter of heat. This heat is given off to the atmosphere mainly in the area just south and east of the Grand Banks. Recent studies by Jacobs (1941) are showing that, in this area and in the corresponding region of the North Pacific, the air is absorbing great quantities of heat from the sea surface and that this heat transfer has an important effect on the wind system.

The heat exchange due to conduction is, of course, relatively small, but that due to evaporation is surprisingly great. As the relatively dry westerly winds leave the continents, they take up quantities of water vapor, evaporating from the sea surface as much as one centimeter per day in winter. Since, at this season, the air is much colder than the sea, the water vapor is carried aloft rapidly by convection, clouds form, and energy is released through condensation.

Meteorologists have realized, for some time, that the general wind system could not be accounted for on the basis of a single heat source at the equator and cold sources at the two poles. Thus, they have been searching for secondary heat and cold sources.

Through recent studies on the borderline between meteorology and oceanography, it now seems clear that a most important heat source is to be found in mid-latitudes, where the warm waters, carried by the currents away from the equator, meet the relatively cool and dry westerly winds flowing off the continents. Such, in all probability, is a most important cause of the storms which form in the North Pacific and which later give us our weather. In the same way, the Gulf Stream area off this coast and to the northeastward is a breeding place for storms which move on eastward towards Europe.

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MAURICE L. HUGGINS, Eastman Kodak Company, Rochester, N. Y.: *Thermodynamic Properties of Solutions of Long-Chain Compounds.* (A. Cressy Morrison Prize Winner, 1941.)

The great and rapidly increasing industrial importance of systems containing long-chain compounds needs no emphasis. In spite of their importance, however, most of the progress that has been made in correlating the properties of these systems with their compositions has been made empirically. Much evidence has accumulated to show that many of the theoretical relationships applicable to systems containing only small molecules cannot be applied, without substantial modification, to systems containing giant chain molecules. This has been especially true with regard to solutions. There has been no quantitative theory of the thermodynamic properties of these solutions—those properties (vapor pressures, osmotic pressures, freezing points, boiling points, solubilities, formation of gels and absorption of solvents by them, etc.) which depend directly on the activities of the component substances.

In the present paper, this lack is remedied. Using well-known statistical methods, equations for the activities are derived and shown to be in good agreement with the considerable amount of experimental data in the literature which is suitable for testing them. One can now proceed with confidence, therefore, to apply these equations to other systems than those tested and to all properties (such as those mentioned above) depending on the activities.

The equations derived are also applicable to solutions containing only molecules of small molecular weight. They furnish a means of calculating the magnitude of the effect of the volume, shape and flexibility of the component molecules on the entropy or randomness contribution to the activities—effects usually neglected (for want of a satisfactory theory) but often far from negligible.

OTTO H. HAAS, The American Museum of Natural History, New York, N. Y.: *The Vernay Collection of Cretaceous (Albion) Ammonites from Angola.* (A. Cressy Morrison Prize Winner, 1941)

The Vernay collection of Albian ammonites from Hanha, near Lobito, Angola, contains more than 1,000 specimens, of which almost 700 are sculptured, normally coiled types. They form the basis of this report. The remainder of the collection, consisting of more or less smooth, normally coiled and of abnormally coiled types, will be discussed in later reports. Seven genera, represented by 91 different forms, 61 of which are new, are recognized among the forms treated here.

Although the Lobito fauna has, since 1888, been studied by several different authors, no one has previously had so extensive a collection upon which to base his conclusions. As a result, the present study has rendered possible the elucidation of new facts of importance in the interpretation and understanding of almost every species recognized. Furthermore, the material is such as to allow the description of the species and varieties not only from a purely morphologic point of view, but also from the point of view of the ontogenetic development of the individuals in each species. This latter information has permitted the recognition of phylogenetic trends which are summarized in the concluding chapter of this report.

The richness of this collection is strikingly illustrated by the fact that though not a single specimen referable to the genus *Dipoloceras* has previously been reported from this area, there are 69 specimens, representing 5 different forms, of which 2 are new, here referred to this genus. Likewise, there are more than 300 individuals referable to the genus *Hysteroceras* as contrasted with one specimen figured and a few others mentioned in the previous reports. Twenty-two different forms, 16 of them being new, are recognized among these 303 individuals. It is noteworthy that the majority of these forms appear to be dwarfed, or otherwise slightly varied, local races of species occurring in the European Gault (Albian) deposits.

The previously little known genus *Neokentroceras* is represented by 117 specimens referred to 14 species and varieties, 9 of which are newly described. The greatest number of forms, 24, of which 19 are new, is recognized in the genus *Pervinquieria.* With the exception of *P.arietiformis,* each of these forms is, however, represented by only a few specimens. The representatives of the former species include several complete individuals which attain diameters up to 31 cm. Particularly interesting, also, are some species of this genus, in which the external tubercles of the ornamentation are so strongly developed as to be well described as "horns."

Probably the most characteristic genus of this fauna is *Elobiceras,* represented by 127 specimens included in 18 forms, 11 of which are new. Of particular importance is the occurrence of the true *E.elobiense,* the genotype, which has previously been known only from a single specimen, inadequately described and figured by Szajnocha in 1885.

The other two genera in the fauna, *Prohysteroceras* and *Neoharpoceras,* are not as abundant as the others, the former being represented by 19, the latter by 4 individuals; 6 forms, 2 of them new, are distinguished within *Prohysteroceras* and two, both new, in *Neoharpoceras.*

Another contribution, made possible by the richness of this collection, is the description and illustration of the suture lines of the majority of the species recognized. Most of these, particularly those of *Elobiceras* and of the closely related *Pervinquieria arietiformis,* have been hitherto unknown, or but very incompletely known.

The concluding chapter of the report comprises a "Palaeontological Résumé" of the evidence elucidated by the study of this part of the fauna. First, the problem of possible zonal differentiation within the Albian of Angola is discussed. Observations made in the laboratory prove that *Neokentroceras* and the group of "unkeeled" *Hysteroceras* do not occur together. According to Spath (1934), the former occur at a lower horizon than the latter. Should this zonal sequence be confirmed, the intricate intergeneric resemblances, here shown in a diagrammatic chart, would permit the assumption of certain phylogenetic relationships between the genera studied so far. These would involve some radical revisions of hitherto prevailing concepts, particularly with regard to the position of the genus *Neokentroceras.* A polyphyletic derivation is indicated for almost all of the genera, and for some groups within them. No evidence suggesting caenogenesis (=proterogenesis Schindewolf) has been found.

Enrique E. Ecker and L. Pillemer, Institute of Pathology, Western Reserve University, Cleveland, Ohio: *Studies on Complement.* (Awarded Honorable Mention, A. Cressy Morrison Prize Competition, 1941)[1]

Complementary activity has been studied for more than forty years but complement has defied all attempts at isolation. Its properties are only partially known and its structure has not been determined. Indeed, Brocq-Rousseu and Roussel recently stated that "complementary power comprises all the activities of unheated serum."

Complement is usually defined as the non-specific portion of fresh serum, which is not increased by immunization, and which, when added to sensitized cells, results in their destruction. In other words, *complement is still defined in terms of its function and not in terms of its constitution.*

The power of complement to hemolyze sensitized red blood corpuscles is only one of its many properties. Among others are: 1. the lysis of certain sensitized bacteria; 2. the opsonization of certain bacteria; 3. the activation of thermostable immune opsonins; 4. the acceleration or augmentation of the aggregation of certain antigens by their homologous antisera, whether the antigen is a bacterial suspension, a protein in solution, or a suspension of erythrocytes; and 5. the capacity to kill bacteria (bacterial action) in the absence of bacteriolysis.

It has been claimed that there is evidence showing that 6. a thermostable constituent of complement operates in the destruction of viruses by immune sera. It has been postulated that 7. a toxin-antitoxin complex binds complement *in vivo,* and that, by this mechanism, the toxin is neutralized. Furthermore, 8. complement has been said to be associated with the coagulation processes of blood. However, recent evidence appears to invalidate this last contention. 9. There seems to be a relationship between the sedimentation rate of erythrocytes and complementary activity, i.e., a fast sedimentation rate was found to be related to a low complementary activity. 10. Complement has even been claimed to be associated with muscle contraction.

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[1] This paper, presented at the Conference on "Immunochemistry," will be published shortly, with others on that subject, in the Annals.

It should be pointed out that complement, because of its multiple components, may be found to promote one of the aforementioned functions while failing to promote others; e.g., a deficiency of one or more of the components of complement may prevent hemolysis without affecting phagocytosis. Therefore, any activity of complement is determined by the substrate upon which it operates; and it thus becomes necessary in defining complementary activity or titer, to define also the substrate employed. In this paper, unless otherwise noted, the substrate referred to is sheep red corpuscles sensitized with rabbit anti-sheep red cell serum.

Nothing definite is known concerning the active principle or principles in serum responsible for complementary activity or the mode of its action. At various times, it has been suggested that complement is a simple chemical agent like oleic acid, a lypolytic enzyme, a proteolytic enzyme, a catalyst, a peptidase, or, more commonly, a physico-chemical state or a colloidal attribute of fresh serum.

These theories, however, do not explain the fact that complement can be separated into two or more functionally distinct components, each of which is essential for the exhibition of its activity.

G. MARSHALL KAY, Columbia University, New York, N. Y.: *Development of the Northern Allegheny Synclinorium and Adjoining Regions.* (George Frederick Kunz Prize Winner, 1941)

Analysis of the stratigraphy of the Paleozoic sediments in the northern Allegheny synclinorium produces a number of conclusions. Gross deformation was downward through early and middle Paleozoic times. The greatest thickness of sediment is in the terrigenous semilenses (deltageosynclines) that represent depressions accompanying the Taconian and Acadian movements. The Adirondack and Cincinnati axes had arches for limited times, were hinges of oscillation, and were temporally more depressed than the central part of the Allegheny belt, but less than the Michigan autogeosyncline and Champlain miogeosyncline. There can have been synorogenic, eustatic and isostatic movements, but they are not conclusively shown. The southern Allegheny belt is partially concealed beneath the Appalachian allochthone, the marginal thrust of which developed along the Adirondack axis. Appalachian folds extend into the northern synclinorium, paralleling the Appalachian structural front, which has the trend of deltageosynclinal isopachs in the north, and of the margin of the Virginian miogeosyncline in the south.

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ELECTED DECEMBER 1 AND 19, 1941

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Deats, William, Retired, Private Research, Paleobotany, Barryville, N. Y.

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Gaudin, A. M., E.M., Sc.D., Richards Professor, Mineral Dressing, Massachusetts Institute of Technology, Cambridge, Mass.

[1] Shoemaker, Norman Clifford, M.D., Staff Member, Biology and Chemistry, Doctor's Hospital, Washington, D. C., instead of Doctor's Hospital, New York, N. Y., as reported in Transactions, Volume IV, No. 2—Ed.

Gaylor, Peter J., B.S., LL.B., Chemistry, Patent Attorney and Engineer, Union, N. J.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 4 FEBRUARY, 1942 No. 4

SECTION OF BIOLOGY[1]

PROFESSOR THEODOSIUS DOBZHANSKY, Columbia University, New York, N. Y.: *Races and methods of their study.*

> Will he not fancy that the shadows which he formerly saw
> are truer than the objects which are now shown to him?
> Plato

Science is not a body of unalterable dogma. It is a growing; changing; at times, senescent; and then, again, rejuvenating organism. Theories and methods start as revolutionary innovations, gain general acceptance, and eventually become impediments to discovery, if applied outside their proper sphere. History of systematics, and particularly of the race concept as a systematic category, are illustrations of this cycle.

There are, in round numbers, 1,100,000 described recent species. Their actual number is hardly less than 1,500,000 and probably more. The human mind is powerless to cope with so tremendous a diversity, except with the aid of the device known

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[1] No meeting was held in January by the Section of Geology and Mineralogy. The Section of Anthropology met on January 26. A paper was delivered by Dr. W. Duncan Strong, entitled "Recent Archeological Research in Peru." No abstract was received.

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as hierarchic classification. In biology, this device was first contrived and applied by Linnaeus, and it continues to be applied to our day. We marvel at the scientific insight of Linnaeus who selected species as the basis of classification, the category of species being the only category above the individual, which, as we now know, has a high degree of objective reality. Species are combined in genera, genera in families, families in orders, etc. These higher categories are arbitrary in so far as it is a matter of convenience as to which subdivision is to be regarded a genus and which a family. They are not arbitrary in so far as the dividing lines between them are made to coincide with the actually existing discontinuities among living things.

Species was the elementary group in Linnaeus's system. Below species were only individuals. Very soon, it was noticed that species are, in turn, subdivided into groups of individuals possessing common characteristics. These groups were termed races or varieties. Linnaeus himself was, of course, aware of the existence of human races, four of which he recognized and described. Very presently Buffon, the father of anthropology, was studying human races in more detail. The eighteenth century of Linnaeus and Buffon was the time of encyclopedic philosophies, of belief in powers of Reason, of rapidly expanding knowledge of new lands, and of supreme confidence in the orderliness of nature. There appeared to be no ground why the methods of describing and studying races should differ from those used to describe species, genera, or other groups. Despite many refinements in the methods of systematics introduced since Linnaeus and Buffon, all categories are still treated alike in principle. The methodology of systematics is determined, first and foremost, by the practical need of pigeon-holing the museum materials in a way that makes them available for further study. But, at its pinnacle, systematics becomes an analytical science intent on discovery of basic regularities underlying the observed facts.

To begin with, systematists were occupied with sorting out complexes of specimens which resembled each other more than they did specimens of other complexes. The complexes were usually designated species. This stage in the development of systematics has not been overcome to our day in the little known groups of animals and plants and in the insufficiently studied faunas and floras. As more and more abundant materials became available, and as the preliminary orientation work in the better known groups approached completion, more discrimination became necessary. Knowledge of the geographic distribution, variation and ecology, usually, though not always, permits us to decide whether two complexes of specimens represent distinct species or races of a single one. Systematists are seldom able to arrange experiments that would demonstrate directly whether the complexes with which they work are or are not prevented by reproductive isolating mechanisms from exchanging genes with each other. The criteria which advanced systematists are using to distinguish species from races are, however, ways to discover by inference whether such an exchange of genes takes place in nature. We know that in the progression from race to species the evolutionary process accomplishes a biological transformation of utmost importance. Races are open systems that exchange genes or are capable of exchanging them. Full fledged species are closed systems that are debarred from exchanging genes by reproductive isolation. The transition from race to species is gradual. Reproductive isolation takes time to become established. Hence, in some instances, it remains arbitrary whether a population is considered a species or a race. This difficulty is inherent in the evolutionary process, but it does not follow that species, in general, are arbitrary entities. At our time level, some groups have unquestionably reached the stage of separate species and other groups are only racially distinct. Still others are in the process of transition. (No other but purely morphological criteria are, however, applicable, when series of forms succeeding each other in time are compared. Dividing an unbroken chain of forms into species or races is a matter of convenience only.)

Racial studies have been carried furthest on human races, on those of certain domesticated forms, and in a few most favored groups of wild animals and plants. Samples of individuals inhabiting a certain region are taken; as many of their characters as practicable are examined; and the averages and the limits of variation of these characters are determined. The result is an ideal racial type or standard. Any individual, unless it is a species hybrid, belongs to some one and only one species. It is assumed that any individual must belong to some one race, namely, to that to the standard of which its characteristics are closest. Individuals which show obvious mixtures of characters of two or more races are dismissed as "intermediates," or considered, frequently on entirely insufficient grounds, to be race hybrids. This method of racial studies is evidently similar in principle to that applied for studying species; *i. e.*, species may be described in terms of ideal statistical types toward which the individual variants gravitate. There is no doubt that this method is convenient for a preliminary orientation in the observed diversity of racial variants, as well. It is with the aid of this method that anthropologists have arrived at a general view of the subdivisions of mankind and, to a certain extent, of their historical interrelations. But this method happens to be misleading when used to analyze the nature and causes of racial variability. Its indiscriminate use has made possible the pseudo-scientific theories of racialism which have played so sinister a role in the preparation of the present world cataclysm.

Before the advent of Mendelism, biologists had believed that heredity is transmitted through "blood." Different hereditary "bloods" were supposed to mix even as alcohol commingles with water. If this view were correct, the hereditary endowments of children would be always a compromise between those of their parents. Brothers and sisters would be genotypically alike. Any sexual population, regardless of how mixed its origins may be, would become more and more uniform with time; since the only source of diversity, after a thorough mixture of the "bloods," would be the new variability arising through mutation. "Pure races" would eventually result, and any individual would in a very real sense be a child, not only of his parents, but of his race as well. Statistical averages would give an adequate description of the "race" as a population.

It is, however, known that heredity is transmitted, not through mixible "bloods," but through discrete and segregating genes. Hereditary variability, in a large sexually reproducing population, decays slowly or not at all. Children inherit only one half of the genes present in either parent. The genotypes of brothers and sisters (identical twins excepted) are different from each other and from both parents. In view of the large number of variable genes and the considerable number of alleles each of them is capable of producing, the variety of genotypes that may be formed in a population is so enormous that the genotype of any one individual living or having lived is unlikely to recur in any other individual. No groups of individuals with identical hereditary endowments can be formed. The concept of "pure race" in sexually reproducing organisms is absurd.

The use of statistical averages to describe the presumed ideal racial types logically implies the existence of "pure races." This method of desorption may suffice for cataloguing the major racial subdivisions, but it is completely misleading when applied to analyse the nature of races. Races are populations of a species differing in frequencies of certain variable genes. Scientifically rigorous description of races can be done only in terms of gene frequencies encountered in the populations.

Since the transition from race to species is gradual, it may be asked why the methods of studying the former must be so different from those used for the latter. The answer is that the quantitative differences between races develop step by step to become qualitative species differences. Accordingly, the classic methods of racial studies have a semblance of validity, when applied to very distinct races that are about to become species. The same methods are invalid where less differentiated races are concerned. Finally, attempts to discriminate between racial types supposedly coexisting within a single panmictic population are patently fallacious, unless the presumed "racial type" is the product of a single gene difference or the population is a race mixture of a very recent origin.

Techniques and methods of race studies based on estimation of gene frequencies are rapidly developing. The remarkable investigations on the blood groups in man (recently ably summarized by Boyd) may be quoted as an example. The best known gene is that responsible for the four earliest recognized blood groups O, A, B and AB. This gene has three easily distinguishable alleles: A, A^B and a. Except for certain Indian tribes, which are supposed to be uniform in their blood composition, all human populations were proved to contain the three gene alleles. The frequencies of these genes are, however, by no means uniform throughout the world. Thus, the gene A^B is rather rare in Europe, frequent in Central Asia, and again relatively rare in Australia. The gene A is common in Australia but reaches a low frequency in Western Europe, e.g., in Ireland. Certain Indians (especially Blackfeet) have an exceptionally high frequency of A. A situation similar in principle is presented by the geographic distribution of the gene arrangements in the third chromosome of *Drosophila pseudoobscura* and in some related species.

It appears that the study of the geographic variation of a single human gene, that of the blood groups, is capable of giving more insight into the nature of the human races than decades of mensuration and computing racial averages. Yet, human populations differ assuredly not in blood groups only. There are hundreds, and possibly thousands, of variable genes in man. Concerted efforts to isolate and to study the distribution of these genes must be made. This is, admittedly, a slow and difficult task. Its slowness and difficulty does not justify, however, formulation of conclusions based on faulty data. The methods used by anthropologists so successfully, in the past, have not completely outlived their usefulness. The limitations of these methods must, however, be understood.

Systematists in most groups of animals and plants cannot be expected to engage in experiments necessary to reveal the gene elements of variation in their materials. Such a project would be clearly impractical, and, fortunately, it is not necessary. Systematists are bound to continue their important and fascinating studies on the collections of specimens preserved in museums.

But they can profit by taking the basic concepts of modern genetics into account in their daily work. For certain refinements in their techniques suggest themselves.

Many experiments on hybridization had been done before Mendel. Some of the early experimenters (Kolreuter, Gartner, Naudin and others) have come within a hair's breadth of the discovery subsequently made by Mendel. The essential difference between their approach and that of Mendel was (we see it clearly in retrospect) that the latter made quantitative studies on the behavior of *separate* characters in the hybrids, instead of trying to classify the individual hybrids according to the *combinations* of characters. Now races differ from each other mostly in several, and, very frequently, in many genes. Genes are discrete and often independently varying units. There is some exchange of genes going on between most races of a species, unless the races are completely isolated geographically. Any systematist with a broad experience in racial studies knows that different characters distinguishing races may, to a certain extent, vary independently. Take the human populations inhabiting the continent of Europe. The stature, the cephalic index, and the pigmentation are only weakly correlated. Anthropologists and systematists will profit by centering their attention on the distribution of separate, well chosen characteristics in the populations that they examine. Discrete, alternatively variable characters are especially valuable. There is certainly no one-to-one correspondence between genes and characters. "Characters" are abstractions which we conceive more broadly or split up into their apparent components, as the exigencies of our studies may demand. Such "characters" in man as the cephalic index and the stature are obviously determined by many genes. They are genetically complex. Nevertheless, a character may be less complex than the organism as a whole. A character is merely an outward sign of the genetic mechanism, which, in conjunction with the environment, determines it. Hence, by studying the behavior of the variable characters, one-by-one, we make the nearest practicable approach open to us, without actual genetic experimentation, to examination of the genic variation. It is a sort of a short cut, a way to make reasonable inferences and approximations. Methodologically, it is similar to the inferential judgments used by systematists to distinguish races from species.

At present, many systematists are preoccupied with the detection of racial or subspecific "types" to which they can attach Latin names, valid under the priority rules. This procedure, patterned entirely on the methods of describing species, genera, and other groups, should be retained for cataloguing purposes. The study of the behavior of separate characters should supplement, rather than supplant, the classic procedure. As pointed out above, the racial "types" are really fictions. But these fictions become more and more useful pragmatic devices, as the differentiating races approach the species; less and less tenable, as we descend in the scale of racial separation. This is because the racial gene complexes become more and more differentiated *as complexes*, when a race segregates itself from other races composing the species. Few biologists have doubted the real existence of the negro and the white races, despite the presence of numerous intermediates between the two, and despite the fact that these races are very far from the degree of separation which could conceivably be deemed to be specific. On the other hand, the distinctions between the Nordic, Alpine, and Mediterranean races are uncertain, despite the efforts of numerous anthropologists. The exchange of genes between negroes and whites was, until recently, limited to a relatively narrow geographic zone. The exchange of genes between the subdivisions of the white race was, and is, extensive. It is quite probable that some genes of the paleolithic man persist in certain European populations, but it is misleading to label individuals possessing certain facial features as belonging to a race of paleolithic survivors. Only in terms of the distribution of gene frequencies, shall we be able to make sense of the differences between local human populations.

It seems not too much to ask of the anthropologists and the systematists that they remember that the achievements of genetics are quite pertinent to the subject matter of their studies. Surely, this reminder works both ways. Geneticists are frequently at fault in their ignorance of the attainments of the descriptive biological disciplines. United, we shall make good progress. Divided, we run the risk of directing our work into the blind alleys of esoteric specialization.

SECTION OF PSYCHOLOGY

JANUARY 19, 1942

LOIS BARCLAY MURPHY, Department of Psychology, Sarah Lawrence College: *Patterns of Spontaneity and Constraint in the use of Projective Materials by Pre-school Children.* (This lecture was illustrated by lantern slides, photographs and paintings.)

In the course of my studies of sympathetic behavior in pre-school children in 1932–34, I found that most pre-school children, who were free to respond spontaneously, were sympathetic to situations which they understood, while the children who had low scores for overt sympathetic responses tended to be those who were also inhibited in other aspects of overt behavior normal to to children at that age. This drew my attention to the problem of spontaneity as such. I also found that children of a lower-income group at a day nursery were more spontaneous in both affectionate and aggressive behavior than children at the university nursery school, where the main study was made. This focussed my interest on questions of how the "family experience" patterns the development of spontaneity and control in different children.

During the last five years at the Sarah Lawrence nursery school, I have been studying this problem along with some others, using a variety of materials; chiefly paintings, finger paintings, Rorschach records, blocks, play with sensory materials—both visual, tactual, and auditory, and miniature life toys. Children were never coerced into participating in the experiments. Whenever possible, the experiments were terminated by the child. They averaged about 45 minutes in length. Since the purpose was research, not therapy, activity of the experimenter was limited to giving responses only when needed for rapport or for clarity of the record. The discussion is based on certain aspects of the records of the toys, finger-paintings and cold cream. The toys included five boxes with materials for house play, a village, farm animals, wild animals, and soldiers. Each group of materials had been suggested by activities of the children during their free play. Both stenographic notes of behavior and photographs of structures were made as a record of the child's activity.

If we inspect the records of free play with miniature life toys from the point of view of spontaneity and constraint, the following main types of play with the toys were easily distinguishable: (1) exploratory play, when the child only names, inspects, or handles the toys in a "getting-acquainted way"; (2) arranging a static pattern with the toys, either of a rigid and meaningless sort, or a conventional representation, such as a house; (3) organizing the toys from moment to moment in various ways, which serve as a vehicle for fantasy of the moment.

The way in which the child plays with the toys reveals his feeling about various everyday experiences: a bed is a soft cuddly place that you enjoy, to one child; to another it is where you are sent when you are bad.

The toys were used in two ways, i.e., with the addition of sixteen large blocks from the nursery school, and without these blocks. Patterns of freedom and constraint (in addition to the usual differences of tightness, looseness, solidity, openness), appearing when the blocks were used with the toys, included the following:– enclosures with much or little room to move around in; high or low barriers (in one case, a double barrier around the enclosure within which the child played kitten); enclosures with large openings or small ones, with few openings or many, closed on the top or open on the top.

I have listed these to give some basis for comparison of normal, relatively unselected children with children in clinic or therapeutic situations. Fears, aggressive wishes, conflicts with parental authority are acted out along with fun-times. It is only the anxiety, which leads to preoccupation with one persistent theme, that is a source of inhibition of spontaneity.

Anxious repetition of the same theme is a less common form of inhibition in this group, however, than the constraint evident in rigid structures such as tracks, fences, and other enclosures;

mechanically set-up houses, made in the form of a square or oblong with exact lines rigidly adhered to. Structures of this sort are made without any evidence of spontaneous play before, during, or after the structure is produced, by children who lack freedom to express fresh ideas or feelings, but do not necessarily show motor inhibition. Motor-inhibited children simply do not play with the toys at all, on initial sessions, or until they are released from motor inhibition.

Without attempting to give any complete analysis of finger-painting, the following most obvious patterns are illustrated by the paintings presented here:- freedom or inhibition in choice of color; use or mixing of color; movement; contact, or texture responses. The free child uses any or all colors, is not afraid to mix them to make fresh nuances. His movements are varied, non-symmetrical, non-repetitive. His contact may show everything from rubbing, patting, smoothing, wiping, to scratching and slapping the paper. Different areas of constraint lie back of such differences, as being afraid to use certain colors, or afraid to mix them; making repetitious vertical or horizontal movements or only symmetrical ones; or only rigid patterns such as fences or walks. Few children of course are completely free or completely inhibited. The sequences and patterns of spontaneity and inhibition are of chief interest.

Over the period of three years, during which many of the children were observed, some children showed long-time sequences from markedly constrained patterns in painting to markedly spontaneous use of paint. Others began spontaneously and became more constrained as time went on.

In numerous cases, it was possible to trace direct connections between changes in the family and changes in the spontaneity-constraint patterns of the children.

In many cases, the same basic spontaneity-constraint pattern appeared through several media. Differences from one medium to another may appear and point to the desirability of finding a basal line for each child in reference to spontaneity, by discovering in which medium he is most free. Forthcoming papers will discuss these differences in "areas" of spontaneity in relation to the temperament of the child. Some three year olds are most spontaneous with sensory materials. Others are most responsive to situations calling for large-muscle activity. Others are most spontaneous with materials which can be manipulated manually, while still others are freest in dramatic situations, and still others respond most to music. Lack of spontaneity in one area, such as building blocks, or participating in group play, does not necessarily mean that the child lacks spontaneity generally. Different combinations of such areas appear to be related to temperament and abilities, as well as family experience.

My discussion so far, has been entirely on the level of description of simple objective differences, easily visible by an untutored observer of paintings and photographs of structure produced by the child. At another level, we may consider the use of symbolization by this group of normal children. Certain objects were used with different meanings by different children.

The fact that, at this age, symbolization of this sort often represents, in itself, an evidence of inhibition (that is, fear of dealing with a situation directly) is suggested by the fact that numerous children begin with symbolic handling of aggression and become progressively more open.

We find the following expressions of inhibition or constraint:- refusal to use certain materials, colors, or ideas; being limited to constrained patterns with toys, blocks, paints; indirect handling of conflicts or wishes by subtle forms of symbolization; and preoccupation with a certain idea surrounded with anxiety. Sequences and patterns of spontaneity and constraint may directly reflect contemporary areas of freedom and constraint in the family life; or they may reflect a distillation of sequences of experience through periods of years or shorter periods. These cultural sources of the child's patterns of spontaneity and constraint cannot be separated from the child's own sensitivity and adaptive patterns. Given similar external pressures or constraints, different children develop different patterns of spontaneity and constraint, depending upon their different temperaments and abilities. Further studies will deal with the relation between these experiments and the Rorschach records of the children, and with the relation of pre-school patterns of spontaneity and constraint to the children's later development.

SECTION OF PHYSICS AND CHEMISTRY

JANUARY 9 AND 10, 1942

Conference on "*Protein Hormones of the Pituitary Body.*"

The Section of Physics and Chemistry held a Conference on "Protein Hormones of the Pituitary Body," as the second in the series for the Academic Year of 1941–1942. Doctor H. B. van Dyke, Squibb Institute for Medical Research, New Brunswick, N. J., was the Conference Chairman in charge of this meeting.

The program consisted of the following papers:

"Introduction," by H. B. van Dyke.

"Criteria of Purity of Proteins," by Theodore Shedlovsky, Rockefeller Institute for Medical Research.

"Hormones of the Posterior Lobe of the Pituitary Gland," By George W. Irving, Jr., Rockefeller Institute for Medical Research, and Vincent du Vigneaud, Cornell University Medical College.

"The Follicle Stimulating Hormone of the Anterior Lobe of the Pituitary Gland," by Bacon F. Chow, Squibb Institute for Medical Research.

"The Luteinizing Hormone of the Anterior Lobe of the Pituitary," by H. L. Fevold, Harvard Medical School and Harvard University.

"Lactogenic Hormone and Mammogen," by Abraham White, Yale University School of Medicine.

"Anterior Pituitary Hormones Affecting Growth and Metabolism," by C. N. H. Long, Yale University School of Medicine.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1942

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1942, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS:

(1) Eligibility. Membership in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1942, to the Executive Secretary of The New York Academy of Sciences at The American Museum of Natural History, Central Park West at 79th Street, New York City.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded

the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1942 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,
Central Park West at 79th Street

New York City

DUNCAN A. MACINNES,
Recording Secretary

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ELECTED JANUARY 22, 1942

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 4 MARCH, 1942 No. 5

SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 2, 1942

PROFESSOR HORACE ELMER WOOD, 2ND., University of Newark and The American Museum of Natural History: *Problems of our Continental Tertiary.*[1] (This lecture was illustrated by Kodaslides).

The late W. D. Matthew (1915) contributed an essay on "The Tertiary sedimentary record and its problems" to the Dana Memorial "Problems of American Geology." It is interesting to compute the extent of progress since the start of the first World War, as we are now, a generation later, fairly launched on an even greater war. This essay remains the most readable general account of the American continental Tertiary and still contains much that the specialist can profitably read. Matthew was much exercised to convince his reader that the continental deposits were

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[1] The field work reported here was assisted by grants from the Penrose Fund of the Geological Society of America and the Marsh Fund of the National Academy of Sciences.

NOTE: This number contains the Announcement of Publications for 1942.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 4, No. 5, March, 1942.

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flood plain and aeolian, in contrast with the lacustrine dogma of last century. This battle is won at last and need not detain us longer. Perhaps we have not yet fully caught up with Matthew's analysis of the magnitude of the direct and indirect contribution made by volcanic ash to the western sediments, but we are moving in that direction. His centrifugal distribution of life from the Holarctic land mass out the three divergent prongs of South America, Africa, and Australasia has become a basic part of our thinking, now largely divorced from such theoretical elaborations as homotaxis and the necessarily peripheral location of primitive survivors.

Matthew protested against correlating faunal lists mechanically, by the degree of similarity or dissimilarity, without regard to the reliability of the identifications or the degree of completeness of the material on which the names were based. His warning deserves an audience today, although professional practice has improved considerably, partly due to Matthew's protests. Intercontinental correlation has made only modest advances since this time. We know about the same things but with a little more confidence due to additional confirmatory evidence. The Mongolian discoveries have provided new lines of evidence which have permitted more satisfactory correlation either with the European record, or with our western deposits, or with both.

This generation has seen the rise, and, perhaps, the decline of the Taylor-Wegener hypothesis of continental drift. In contrast with the unconsciously misleading use made by Wegener of paleontological arguments drawn from standard texts (such as Osborn's "Age of Mammals," 1910), no mammalian paleontologist, so far as I know, has been converted to "Continental Drift." For the specialist on the Cenozoic, continental drift would raise far more, and more serious, new difficulties than the old ones which it would apparently solve. This, of course, has no bearing on whether the continents might have drifted apart before Cenozoic time, or, for that matter, whether they may have alternately secured a divorce and then nestled affectionately together again. But the mammalian record contradicts any such rapprochement during the last sixty million years or so. However, there is no question that this hypothesis has stimulated observation as well as discussion and therefore illustrates how even a probably fallacious generalization may proove stimulating.

Some progress has been made in attempts to intercorrelate the marine and continental sediments, and additional solid progress may be anticipated. Early slipshod work in Florida and the Texas Coastal Plain has been or is being straightened out, important evidence has been obtained from these areas and from California, and more may be expected. Nevertheless, it is not all peaches and cream, and the new titanothere from Mississippi (Sullivan and Gazin, in press) appears to be of very late Eocene aspect according to the continental standard, but associated with middle Eocene marine fossils. This recalls a similar embarrassing quandary regarding the not too primitive titanothere, *Brachydiastematherium transsylvanicum*, and the true rhinoceros, *Prohyracodon orientalis*, from the variegated clays of Andrashaza (Wood, 1929). Andrashaza Hill is near the city variously known as Klausenberg, Kolozsvár, and Cluj, which, once in Hungary, then in Rumania, and recently returned to Hungary, is even more uncertain of its ultimate geographic location than is the geologic level of its mammalian fossils. The horizon of these two mammals (which appear late Eocene or even early Oligocene in morphologic character) is underlain by a late lower Eocene marine fauna and overlain by another marine fauna of middle Eocene affinities. This anomalous situation at Andrashaza has been known since the end of the last century, but has usually been ignored or explained away rather than faced squarely. It is, of course, in the middle and upper Eocene that correlation between the marine and continental sequences has been—and remains—most dubious and is largely based on indirect lines of evidence. Nevertheless, these discrepancies are difficult to explain.

It is interesting to compare Matthew's correlation chart (1915, p. 412) with a recent summary (Wood *et al.*, 1941, Pl. 1). There are new occurrences and greater refinement in detail, throughout, but it is striking how little the Puerco and Torrejon, the standard Eocene succession, and the Oligocene to lower Miocene sequence, have changed in a generation. The Paleocene epoch has at last been recognized by the U. S. Geological Survey and other conservatives, and its boundaries have been satisfactorily delimited. The upper Paleocene has become progressively better understood. The Loup Fork, which had only just been recognized as a scrap basket where an apparently precise name operated to prevent precise observation or communication (Osborn and Matthew, 1909) has now been fully resolved into its component parts, and the Pliocene, until recently a hopeless hash (or, if you prefer a pleasanter foreign word, "mélange"), now presents successive faunas showing stages of autochthonous evolutionary advance as well as immigrations from other continents. Thus the Blanco and Hagerman faunas are now recognized as late Pliocene and a new middle Pliocene has been segregated out of the amorphous "Loup Fork."

The dispute over period and epoch boundaries is perennial and remains about where Matthew left it. Most of us would agree that time is continuous and that geological time boundaries are as arbitrary as the distinction between 11:59 P. M. of one day and 12:01 A. M. of the next. Nevertheless, the question seems to exercise an undying fascination and frequently generates heat without light. Legalistically speaking, epoch boundaries are based on the European standard, but there has been almost as much difficulty in securing agreement among European geologists about geologic boundaries as there seems to be, currently, about national boundaries in the same continent. Perhaps, if the larger and more immediate problem should be satisfactorily settled, it might then be possible to secure agreement on whether the Aquitanian is "really" Oligocene or Miocene, and the Pontian is "really" Miocene or Pliocene, but I am not too optimistic. Further complication is furnished in this country by some parochial geologists, whether state officials or self appointed local patriots, who feel that epoch boundaries must "naturally" coincide with the levels that would most conveniently fit the deposits of their particular state.

A school of geologists has claimed that geologic time divisions are "naturally" delimited by diastrophism. This concept, which assumes as self-evident that diastrophic activity in diverse parts of the world jerked along by synchronized fits and starts, represents a surprising reversion toward Cuvierian catastrophism.

(As the rest of this address was illustrated by Kodachrome slides, much material is omitted that would be unintelligible without illustration.)

Correlation and mapping has always been, and doubtless always will be, based to a considerable extent on the general aspect of the rocks. This is particularly true of continental sediments, in which local fossil pockets are often separated by miles of completely barren sediments, and in which the color, the pattern of weathering and erosion, and other lithological peculiarities may be very striking. It is a commonplace that we may be completely misled by such criteria, but we can not avoid trusting to them in the absence of fossils, and, even when fossils occur, the evidence from the sediments themselves should not be ignored. Sometimes, the boundaries apparently indicated by color and other lithological peculiarities are not substantiated by the faunal boundaries established later. This is the case for the contact between the Uintan and Oligocene along Beaver Divide on the south side of the Wind River Basin, and between the upper Eocene Sage Creek formation and the middle Oligocene Cook Ranch of southwestern Montana. Evidently, something happened during Uintan time which was reflected in a sharply changed lithology in these two areas and in the Uinta formation of Utah. A little later, between Eocene and Oligocene, comes the sharpest break in the whole Tertiary continental sequence, with the bulk of deposition shifting from the intermontane basins to the High Plains. Again, deposits demonstrated to be contemporaneous by their fossils may differ completely in lithology, especially if they are a considerable distance apart. There is not the slightest resemblance between the Torrejon formation of the San Juan Basin of New Mexico and the contemporaneous Rock Bench of Park County in northwestern Wyoming; nor between the Arikaree of Nebraska and the channel sandstone of comparable age containing *Diceratherium*, at the mouth of the Madison River in Montana, north of Yellowstone Park; nor between the Madison Valley formation of the same area and the contemporaneous Barstow of the Mojave Desert of California. Or, rocks of similar aspect may be of exceedingly different ages, as illustrated by the striking color similarities of phases of the Chugwater and Lysite of Wyoming; although this may not be pure coincidence, as it has often been suspected that the Lysite colors are directly derived from the Chugwater. In all these and other problems, it would seem that the paleontologist might better call on the sedimentary petrologist for collaboration, rather than try to master that far removed and highly complicated field for himself.

The perennial Wasatch problem has now nearly ceased to be one of fact, and has become one of popular education; that is, of educating the non-specialist geologist. The text books picture the "Wasatch" with a distinctive fauna, which fauna (the Gray Bull) has never been found in the type Wasatch group and is quite probably represented there only by the hiatus below the Knight formation. On the other hand, the books tell us that the Wind River formation overlies the Wasatch and contains two later faunas—those of the Lysite and Lost Cabin members. However, both these faunas and no others are now known in the Knight and, if other workers agree in treating Love's Indian Meadows formation (1939) as the basal member of the Wind River formation, the Wind River not merely correlates with the entire Knight, but also includes a lower member (equivalent to the Gray Bull) which contains what most people have called the "Wasatch" fauna, unknown, however, in the type Wasatch. This leaves out of account the age of the probably late Paleocene Almy and the dubious Fowkes, which are, by definition, part of the type Wasatch group, but hardly belong to the standard Wasatch concept, which is better expressed in the modified form of "Wasatchian," *i.e.*, lower Eocene of the Rocky Mountains.

In recent years, the Green River formation has begun to present a conceivable picture of possible real conditions (especially due to Bradley's work), as opposed to a cut and dried text book lake deposit. The complete separation of the northern and southern lakes has been emphasized by increasing evidence (particularly from the Carnegie Museum expeditions) that the northern lake was completely filled or drained before the southern lake was well started. Interfingering between the Green River and the contemporaneous continental deposits was first shown by Sears and Bradley (1924), and additional cases have been discovered by Jepsen, east of Labarge, Wyoming, and by me, between Boulder and Big Piney, farther north in the same region. The conception of the complicated relations between lacustrine and continental sediments formulated by Nace (1939) will probably win general acceptance in time.

Slim Buttes, in northwestern South Dakota, presents an interesting structural problem. It seems startling to find Great Plains Oligocene apparently folded or faulted into steep dips (Todd, 1895). Winchester (1913) interpreted these structures as large scale crossbedding, distinguishing this old structure from recent slumping, which he recognized around the edge of Slim Buttes. Toepelman (1923) explained the earlier structures by analogy with this slumping, as caused by "pre-Miocene" slumping, and this interpretation seems to be accepted in later literature. This hypothesis requires deep, steep walled canyons cut and maintained in the unconsolidated White River sediments, followed by slumping, in which these (by his hypothesis) weak sediments behaved as sufficiently cohesive structural units to simulate text book fault blocks. How a landslide actually proceeds is illustrated by the Gros Ventre landslide; and slow slumping, by the recent slumping around the edge of Slim Buttes. Apparent support for the explanation of essentially contemporaneous slumping was also furnished by the supposed age of the overlying cliff-forming sandstone, which was tentatively referred, without fossil evidence, to the Arikaree; since this assignment allowed time for erosion and essentially contemporaneous slumping, but little time for diastrophism and peneplanation. A fossil rodent in the Carter County Geological Society museum, collected from the same formation where it caps Chalk Buttes near Ekalaka, Montana, is of late Miocene aspect. This new date for the massive sandstone leaves ample time for diastrophism, followed by peneplanation, previous to the deposition of the caprock.

For more than a generation, *Daemonhelix* has been interpreted, alternately, as a plant growth and as a fossilized rodent burrow. Just as it seemed that the burrow interpretation had finally won by common consent, Lugn reinterpreted this spiral structure as a liane (1941). Certainly, many of the vertical spirals resemble climbers, but one is startled by the change in ecological conditions which Lugn thus postulates for the Harrison, from the currently accepted temperate plains to a marshy tropical or subtropical forest. The corroborative evidence for this new interpretation seems inadequate.

Correlation of the Tertiary sediments may help the structural geologist to solve the problem of the Rocky Mountain basins and their mountain rims. Thus, Lysite Mountain, which lies virtually on top of the southern hook of the Bighorn Mountains, is composed of sediments identical with the Green Cove and Uintan of the Wind River Basin. It is difficult to believe either that the Wind River and Bighorn basins were once filled level with the top of Lysite Mountain, or that there has been post-Eocene differential movement, between the Mountains and the Wind River Basin, that could be measured in thousands of feet (as opposed to movements totalling some hundreds of feet which have evidently taken place). Again, sediments which, so far, have proved unfossiliferous, but are vaguely of White River aspect, lie on top of the Bighorn Mountains, west of Buffalo, at altitudes up to 9,000 feet. One would be rash to fill the Powder River Basin (in his mind's eye) deeply enough with sediment to accord with this and the scattered White River outliers in the Black Hills (*e.g.*, at Lead). However, the almost buried Granite Mountains of central Wyoming, and the rugged topography of the Absarokas partly buried beneath the Aycross formation, do show how mountain ranges may be virtually buried in their own débris, with or without the assistance of volcanic ash. The extent to which the Bighorn

Mountains were actually buried also virtually decides whether we should interpret the anomalous course of the Bighorn River, through the Wind River Canyon, as antecedent (thereby neatly begging the question), as superimposed during a time of filled basins, or as a result of piracy along the Boysen fault. With either of the latter explanations, regional tilt to the northeast would also be a major contributing factor.

It is regrettable but true that no progress can be reported on the exasperating problem of the relatively sudden extinction of the large reptiles at the end of the Cretaceous, and their replacement, rather shortly thereafter (so far as the stratigraphic record indicates—particularly at Polecat Bench, Park County, Wyoming), by an apparently new mammalian fauna with no known ancestry in the Cretaceous.

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SECTION OF BIOLOGY

FEBRUARY 9, 1942

PROFESSOR BRENTON R. LUTZ, Department of Biology, Boston University, Boston: *The Neuro-Motor Mechanism of Small Blood Vessels.* (This lecture was illustrated by motion pictures and lantern slides).

Are the capillaries merely inert thin-walled tubes dependent on the arterioles for their blood supply, or are they independently active, with a capacity to respond individually through nervous and hormonal control? On the answer to this question may depend the correct interpretation of many physiological and pathological conditions, such as muscular activity, increased glandular secretion, the stopping of hemorrhage, inflammation, traumatic shock and Raynaud's disease. Krogh (1919) pointed out that a resting skeletal muscle may have less than 100 open capillaries per square millimeter, while the increased oxygen demand of an active muscle is met by the opening of more than 2500 capillaries in the same cross-sectional area.

Several mechanisms for controlling capillary blood flow have been described. In Krogh's laboratory, Vimtrup (1922) reported the contraction of Rouget cells on the capillaries, which caused folding of the endothelium. In a review of the literature on the regulation of capillary blood flow, Krogh (1929) presented the evidence for the concept of independent contractility of the capillaries. Recent investigators using improved techniques have come to no general agreement concerning this concept or the mechanism involved. Kahn and Pollak (1931), working on the excised nictitating membrane of the frog, noted the protrusion of endothelial nuclei into the lumen, and the longitudinal folding of the endothelium in the region of pericapillary cells. Using the frog and rat, Field (1935) reported capillary closure in two ways: first, the swelling of endothelial nuclei which blocked the lumen of the capillary; and, second, the contraction of pericapillary cells (Rouget cells). Beecher (1936) confirmed Field in studies on the blood vessels within transparent chambers inserted in the rabbit's ear. Using a similar preparation, Sanders, Ebert and Florey (1940) reported the swelling of endothelial nuclei following electrical stimulation of the peripheral end of the cut cervical sympathetic nerve. Clark and Clark (1940) observed the contraction of endothelial cells in the tadpole's tail, but not in the rabbit's ear. They have not seen the contraction of pericapillary cells, but concede that it may take place in certain tissues, such as the nictitating and hyaloid membranes of the frog. The new technique of micro-manipulation, as applied to capillaries by Zweifach (1934), made it possible to study this problem by direct methods. Zweifach (1939, 1940) maintained that capillary blood flow was regulated chiefly by the contraction of small arterioles, and that changes in capillary caliber were merely passive.

In a preliminary report, Fulton and Lutz (1940) stated that capillary origins in the retrolingual membrane are provided with modified smooth muscle cells, which may regulate capillary blood flow in a sphincter-like manner independently of the supplying vessel. The present paper presents further evidence for this view, along with the results of micro-stimulation, recorded by the motion picture camera, which support the concepts of dual innervation of the small blood vessels and of discrete vascular smooth muscle motor-units. Since 1937, Dr. George P. Fulton in our laboratory has confirmed and extended my original work.

METHOD

The blood vessels in the retrolingual membrane of the frog, *Rana pipiens*, with brain and medulla destroyed, were prepared for use after the method of Pratt and Reid (1930). In this membrane, blood vessels, nerves and skeletal muscle fibers are arranged in a thin sheet between two protective layers of epithelium. From the dorsal surface of the tongue, sufficient papillary epithelium and skeletal muscles were removed to expose the membrane beneath. The frog was placed in a Petri dish, containing a glass block cemented to the bottom. The tongue was everted so that the upper surface of the retrolingual membrane rested on the block. Sufficient Ringer's solution was added to cover the membrane. The entire preparation was placed on the stage of the microscope and illuminated by transmitted light.

For use in stimulation, unipolar silver-glass micro-electrodes, one to five microns in diameter at the tip, were made by drawing a glass capillary containing silver wire (Fulton, 1941). The micro-electrode was placed in the field of the microscope by means of an Emerson micromanipulator. Brief faradic currents were passed through the electrode. The minute bubble of gas, which appeared at the tip of the electrode when the current was made and increased in size during the period of stimulation, served to indicate the moment and the duration of the stimulus. A water immersion lens was used, and, with a light-splitting prism, cinephotomicrographs were obtained at magnifications as great as 900 times.

Numerous preparations of the retrolingual membrane were stained differentially with methylene blue (National Aniline Company) for nerve fibers and perivascular cells, by adding three or four drops of a 0.5 per cent aqueous stock solution to approximately 10 c.cm. of Ringer's solution covering an excised preparation. The membrane was fixed in saturated ammonium picrate, mounted in glycerin and sealed.

RESULTS

In the frog's retrolingual membrane, the vascular pattern consists of from two to four arterioles, which enter from each side and ultimately divide into anastomosing capillaries which converge laterally into venules. Examination of methylene blue preparations, stained differentially for perivascular cells, showed that the numerous typical smooth muscle cells of the arterioles became somewhat scattered on the precapillaries. The pericapillary cells, at the origin of certain capillaries, were branched and possessed nuclei arranged obliquely with reference to the capillary wall. Those found farther along the wall possessed longitudinally arranged nuclei. Although the protoplasm of perivascular cells stained poorly, finely-branched cytoplasmic processes were seen frequently. A structural transition was evident from the typical smooth muscle cells on the arterioles, through the branched modified smooth muscle cells at the capillary origins, to the scattered pericapillary cells. In some capillaries there was a large extent of free endothelial surface. The perivascular cells on the venules had no definite arrangement.

The nature and distribution of the perivascular cells were also examined by means of stimulation with a micro-electrode. Most of the perivascular cells on arterioles, precapillaries and certain capillary origins contracted in response to direct stimulation and to indirect stimulation through a nerve. Except for an occasional pericapillary cell, those peripheral to the origin did not contract. The endothelium did not respond to mechanical stimulation, direct electrical stimulation, or indirect electrical stimulation through a nerve. Endothelial nuclei were not observed to swell and block the lumen of the capillary. The perivascular cells on small venules did not contract. Certain perivascular cells on the capillaries could be made to migrate a considerable distance along the wall by successive weak faradic stimuli. Others moved around the vessel, presenting a greater surface view of the nucleus, and creating the impression of nuclear thickening.

Examination of methylene blue preparations, stained differentially for nervous tissue, showed that non-myelinated nerve fibers, branches of the glossopharyngeal and hypoglossal nerve trunks, formed a loosely-meshed plexus, the primary or gross plexus, throughout the retrolingual membrane. Nerve fibers from the gross plexus approached the blood vessels and formed a perivascular plexus consisting of a long-meshed network, the secondary plexus, following the course of the vessels. This, in turn, formed a fine-meshed network, the tertiary plexus, on the vessel wall. The secondary plexus was abundant on the walls of arterioles and precapillaries, but sparse on the capillaries. Ganglion cells were not found in the retrolingual membrane. Many small fibrils from the tertiary plexus became progressively fine and passed beyond the range of vision. Actual nerve endings were not found, although sufficient nervous tissue is present to innervate all the contractile elements. These observations confirm those of Woollard (1926) on the small vessels of the heart of the mammal.

Brief faradic stimulation of small non-myelinated nerves, composing the primary and secondary plexuses in the field of the microscope, gave evidence for smooth muscle motor-units, as well as for the presence of sphincter mechanisms at the capillary origins. Stimulation commonly produced spatially limited diphasic responses, dilation followed by constriction, in a specific vascular pattern. In control experiments, the micro-electrode was placed on the epithelium near a small nerve, but stimulation produced no response unless the electrode was put exactly on a nerve. Fulton and Lutz (1942) showed that, in the diphasic response, the constricted area was frequently only a portion of that originally dilated. Weak stimulation of a nerve sometimes produced only dilation, while strong stimulation of the same nerve might cause only constriction. Although the most frequent response to nerve stimulation was diphasic, a few vasomotor nerves produced either dilation or constriction to all strengths of stimulation. It is conceivable, therefore, that dilator and constrictor fibers are segregated occasionally near their terminations. These observations suggest the concept of discrete vasodilator and vasoconstrictor nerves with different thresholds of electrical stimulation.

In preparations treated with one per cent cocaine hydrochloride, stimulation of the vessel wall produced constriction in exactly the same vascular pattern as that produced by nerve stimulation before treatment. Cocaine made the nerve plexus non-functional, since stimulation of small vasomotor nerves produced no response. Consequently, the limited conducted response implies a non-nervous conducting mechanism, such as a muscle syncytium, discontinuous at the junctions of certain vessels. Our investigation shows that the musculature of the blood vessels of the frog's retrolingual membrane is composed of a large number of discrete smooth muscle motor-units. Extensive contractions of the blood vessels, when a nerve is stimulated, may depend upon the activation of these motor-units coordinated through the nerve supply.

The contracting smooth muscle cells, following either direct or indirect stimulation, may be so completely coordinated that the responding vessels close simultaneously and abruptly. In other instances, portions of the vascular pattern, or of a single vessel, may be out of phase, resulting in one part being dilated, while an adjacent portion is being constricted.

Independent contraction of the capillary origin in a sphincter-like manner was commonly observed. Following nerve stimulation, pericapillary cells, at the origin of the capillary from the arteriole, or precapillary contracted to block capillary blood flow, either partly or completely. The capillary beyond this point always remained open. The contractile sphincter mechanism appears to consist of the modified smooth muscle cells which are stained at the capillary origins in methylene blue preparations. These cells are probably similar to, if not identical with, the contractile type of pericapillary cell reported by Rouget (1873), Vimtrup (1922), Tannenberg (1925) and others.

Spontaneous contractions of small blood vessels, confined to limited vascular patterns and to individual capillary origins, were frequently observed. Thus, smooth muscle motor-units may be rhythmic at times. Rhythmic contraction could be initiated by nerve stimulation, which, in certain instances, produced a contraction in exactly the same limited pattern, previously rhythmic. The cause of the spontaneous rhythmic activity of blood vessels, cut off from the central nervous system, was not determined. No ganglion cells were found in the methylene blue preparations. Furthermore, spontaneous rhythmicity occurred in lightly cocainized preparations, after stimulation of nerve fibers had ceased to give vasomotor responses. This suggests that spontaneous activity is a property of the smooth muscle of the blood vessels and of the modified smooth muscle cells of the capillary origin.

SUMMARY

1. The nature and distribution of the contractile elements of

the small blood vessels in the retrolingual membrane of *Rana pipiens* have been examined by stimulation of vasomotor nerves and blood vessel walls with a micro-electrode, and by differential staining with methylene blue. In addition to the typical smooth muscle of the arterioles, there are modified smooth muscle cells at the capillary origins. Scattered perivascular cells beyond this point were generally inert. The endothelium did not respond to mechanical stimulation, direct electrical stimulation, or indirect stimulation through a nerve; nor did the endothelial nuclei swell to block the lumen.

2. A diphasic response, dilation followed by constriction, was generally produced in a limited vascular pattern by brief faradic stimulation of small vasomotor nerves, in the field under the microscope. The constricted region was frequently only a portion of that originally dilated. Some nerves gave only dilation, others, only constriction. These observations are interpreted on the basis of dual innervation.

3. Syncytial smooth muscle motor-units make up the musculature of the small blood vessels of the retrolingual membrane, since exactly the same limited vascular pattern responded to stimulation through the nerves and to direct stimulation of the vessel wall after cocainization. The perivascular nerve plexus is physiologically discontinuous.

4. Sphincter-like activity of the modified smooth muscle cells at the capillary origin occurred in response to brief faradic stimulation of small nerves, and spontaneously. The capillary always remained open beyond this point. Thus a mechanism is present, under nervous control, which may regulate capillary blood flow locally, even though the endothelium is inert.

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SECTION OF PSYCHOLOGY

February 16, 1942

Doctor Richard Youtz, Barnard College, Columbia University: *The Status or Certain Conditioning Principles after the Eclipse of 'Reflex Psychology.'* (This lecture was illustrated by lantern slides).

This evening, I should like to trace briefly the history of some principles initially discovered on the conditioned response. After that, we will consider a few experimental situations where Pavlovian conditioning itself is unimportant, but where these principles seem to apply.

Interest in the conditioned response began in this country in connection with a kind of crusade to bring objectivism to psychology. It was used as a new and objective technique, but it was also used as an explanatory device in places where, as we now suspect, it did not belong. Although the Pavlovian conditioned response is now little used as a building block in psychological theory, the *characteristics* of the conditioned response have attained a life of their own in application to a large number of different problems. When I speak of "characteristics," I refer to such things as extinction, spontaneous recovery, secondary extinction, and generalization. For instance, these characteristics have been found in learning by reward. They are also useful in describing trial-and-error learning, and the Gestalt transposition experiment. Some of them appear in rewarded verbal learning, and there are indications of their presence in certain simple kinds of concept-formation and, perhaps, in problem solving. I shall not attempt to review the literature on these topics, but will choose typical and, I hope, cogent examples. The literature has been amply reviewed by a number of writers, among them, Razran, Hilgard and Marquis, and Skinner.

The heyday of the conditioned response, as an all-explanatory principle in psychology, was during the 1920's. Watson had used it in his presidential address as a substitute for introspection and presented it as an important technique in the study of emotion, mental disease, the formation of language, and habit structure. During the 20's, the conditioned response spread into many fields as an explanatory principle, before experimental findings offered much sound support. In 1921, Smith and Guthrie published a general textbook which used the conditioned response in place of a more general associative theory. In 1924, F. H. Allport described many of the phenomena of Social Psychology in terms of the conditioned response. In the same year, Burnham's book, "The Normal Mind," presented it as a useful explanatory principle in the field of mental hygiene. These and many others seized upon the new concept and applied it to various theoretical problems.

However, experimental verification did not keep pace with theoretical description. It was found, for instance, that maze learning was not easily fractionated into a series of chained reflexes. In human beings, the desire for money was certainly not inherited, nor was it apparently a second-order conditioned response. The second-order responses set up in the laboratory were too tenuous and transitory to be examples of strong derived drives. Finally, reward and punishment learning (or law-of-effect learning), did not fit the conditioned response pattern and many data were best described in terms of reward and punishment. The Pavlovian picture was found to be inadequate and the first wave of enthusiasm passed.

The law of effect had long been important in trial-and-error situations and had been well-known since E. L. Thorndike's experiments and theoretical formulations, beginning in 1898. Now it was found that Thorndike's Law of Effect and Pavlov's conditioning principles dealt with very similar situations. As they were compared, it became apparent that, while they were concerned with similar problems, they differed as to the particular stimulus-response connection that was strengthened during learning. In the conditioned-response paradigm with S_1, the bell, and S_2, the meat-powder, the animal learns to connect S_1 with R_2. R_1 is ignored. In the Thorndikian paradigm, the connection between S_1 and R_1 is strengthened when S_2 and R_2 are reward. The connection between S_1 and R_2 is largely ignored. The question now arose of whether the S_1-R_2 connection of Pavlov and the S_1-R_1 connection of Thorndike had characteristics in common and acted in the same way in a variety of experimental conditions. Specifically, did Thorndike's rewarded learning exhibit extinction, recovery, secondary extinction, and so on? Since we will deal with these concepts a number of times, I'll describe them briefly. In conditioning, S_1 comes to evoke R_2 through repeated presentations of S_1 and S_2. In extinction, the conditioned responses die out when the S_1, or bell, is presented without S_2, the meat powder. After the weakening of a conditioned response in extinction, it will recover some of its strength, if the experimenter simply waits, and presents no experimental stimuli to the subject. This peculiar property is known as "spontaneous recovery." In "secondary extinction," the extinction of one conditioned response in a particular experimental subject also weakens other conditioned responses in the same subject. If a parallelism between conditioning and reward learning is established, experimental findings on either of these kinds of learning have a much wider area of possible application.

As a result of a large number of investigations, it has been found that rewarded learning *does* exhibit many of the characteristics of the conditioned response. Grindley found that learning occurs if a guinea-pig is rewarded with a piece of carrot when it turns its head to the right at the sound of a buzzer. (This is very similar to Thorndike's experimental findings, with the difference that, in Grindley's procedure, the head-turning movement occurred after a specific buzzer-stimulus.) Grindley also found that this head-turning tendency is subject to extinction, if the carrot-reward is not given. Skinner has made extensive investigations of rewarded behavior. In his experiments, a rat is placed in an experimental box and trained to push down a small lever. Depression of the lever releases a piece of food which constitutes a reward for the rat. The animal soon learns to continue pressing the lever to obtain food. When the food-release mechanism is disconnected, the rat continues to press the lever and the records show a typical extinction curve, with the rat finally making only occasional responses. Skinner has also found that, if the rat is removed from the experimental box following extinction and then replaced after an interval, it again presses the lever repeatedly, showing spontaneous recovery. With this experimental technique, Skinner has studied discrimination-learning and a number of other related phenomena. Using a Skinner-type experimental box, the speaker has found two further Pavlovian principles in rewarded learning, namely, secondary extinction and a rise in the retention curve. In the secondary-extinction experiment, either of two levers can be placed in the box, one horizontal and one vertical. It was found that after the rat had learned to operate each of these levers separately, extinction on one of them would reliably decrease the number of extinction-responses on the other.

The rise in the retention-curve for rewarded learning parallels a similar rise for the conditioned response, which seems not to follow the familiar retention curve for learned verbal material.

Almost all of the above findings have been confirmed by independent experimenters. Because of this functional similarity between rewarded learning and conditioning, the terminology has also changed. The majority of writers on the subject consider them both as forms of conditioning. Miller and Konorski suggest that rewarded learning be regarded as a second type of conditioned response. Skinner considers rewarded learning as more important and describes it as a Type I conditioned response, with the Pavlovian sort as Type II. Hull has derived both sorts of learning from more general associative principles and regards conditioning as a special and limiting case of learning by reward. Schlosberg, on the other hand, distinguishes carefully between the two kinds of learning, while pointing out the similarity of their results. Razran considers rewarded learning as qualitative, and directional conditioning, with conditioning itself, as quantitative and basic. Hilgard and Marquis describe rewarded learning as "instrumental conditioning" in contrast to the classical Pavlovian sort. There seems to be no doubt that rewarded learning is changing its name.

And now, what have been the advantages of the extension of these conditioning principles into a new field? We may use two situations as representative.

IN TRIAL-AND-ERROR LEARNING

The first is in trial-and-error learning. In Thorndike's formulation, the animal is placed in a problem-situation, in which it can make a number of different responses. For instance, there is the cat in the problem-box, in which it has to push a button to open the door to food. Each trial is terminated when the cat makes the correct response and gets food. On successive trials, the cat takes less and less time to arrive at the correct response and finally eliminates the incorrect responses and does the correct one as soon as it is put in the problem-box. This is certainly one of the commonest forms of learning in animals and humans.

Hull has applied conditioning principles to trial-and-error learning and has suggested several questions to be answered by any explanation of it.

1. Why does the organism frequently repeat unrewarded responses? *i.e.*, instead of trying some other response?

2. When a response has been rewarded, why is it not immediately fixed upon and continued without interruption? If an organism is successful on the last response of one trial, why does it make several wrong responses at the beginning of the next trial?

3. What is the order of eliminations of the unrewarded responses?

We can give a qualitative description of trial-and-error learning and answer these questions if we apply principles of reinforcement, extinction, and recovery.

Let's assume that the cat is in the problem-box and three of its responses are: *1.* Jumping up and bumping its head against the top of the box; *2.* Miaowing pitifully; and *3.* Scratching at the release-button. There are, of course, many other responses and gradations of these, but we'll take only a few for illustration.

Let us further assume that this particular cat has most often been successful in escaping from boxes by *bumping its head* against the top. Because of this frequent success, its strongest reaction-tendency is toward bumping its head. It has been successful less frequently when using the method of *miaowing pitifully*, so that this reaction-tendency is not quite so strong. And, finally, it has only occasionally escaped by *scratching* at the side of the box near the door (and the release button), so that this reaction-tendency is the weakest of the three.

When the cat is put into the problem-box, it first bumps its head against the top, because this has been most often rewarded in its previous experience. It bumps its head more than once (Question 1) and, in fact, continues to do so, until the head bumping tendency is weakened by *extinction* to a strength less than that of the miaowing tendency. It then miaows several times. But, while it is miaowing, the head-bumping tendency undergoes *spontaneous recovery*. The cat again bumps its head. The head-bumping and miaowing continue until both tendencies are reduced below the level of strength of the scratching tendency. It then scratches at the door, is released, and obtains a bit of fish which constitutes a reward.

When the cat is put into the problem-box for the next trial, it would, if it were entirely rational, immediately scratch the release button. However, the head-bumping and miaowing tendencies have recovered part of their original strength and the cat again performs those acts. (Question 2.) It reaches the release button more quickly this time, because the head-bumping and miaowing are more quickly extinguished below the level of the successful response. This sequence is repeated for successive trials, until the cat scratches the release button immediately after being placed in the box, and the problem is then considered solved. To answer Question 3, we must know the original strengths of the tendencies and the shape of the curves for successive extinctions and rewards for the particular animal, in this particular situation.

Secondary extinction probably also operates here, reducing the strength of all of the response-tendencies when one of them is being extinguished.

This is a most irrational picture and one's tendency is to say —"This happens in animals, but humans don't follow this pattern. Human beings act rationally." This is extremely doubtful. Let me illustrate with the picture of a man hunting for theater tickets just before he leaves home for the theater. Usually, in the past, the man has put the tickets in his top bureau drawer. Occasionally, he has left them in his vest-pocket; but this time, with special care, he put them in his overcoat pocket.

When he is about to leave for the theater, our friend goes confidently to the bureau drawer. The tickets aren't there. He looks again, more carefully, and still more carefully. Then he goes through his vest pockets. In the meantime, the bureau-drawer tendency is recovering. He looks in the drawer again. After going back and forth several times, he "remembers" that the tickets are in his overcoat pocket and finds them. His behavior is very like that of the cat in the problem-box on a single trial.

The action of the separate variables is difficult to distinguish in the records of an actual trial-and-error situation. However, I have some curves which, at least, show the effects of recovery and extinction. Ten rats were trained to push down a horizontal lever or bar, to get food, and also trained to push a vertical bar sideways to obtain food. Each rat was given 60 rewarded trials on each bar alone. After this training, the rat was put in the experimental box with *both* bars present. The response to one bar, the vertical for instance, is rewarded, while that to the horizontal bar is not. The rat is allowed a *total* of 30 responses a day on both bars together. This is repeated, day after day, until, on one day, 28 out of the 30 responses are made on the rewarded bar—the vertical, in this instance. After this criterion is reached, the situation is reversed, and responses to the horizontal bar are rewarded and those to the vertical not rewarded. Ten rats were run on this schedule for 74 consecutive days. These rats took varying numbers of days to reach the criterion of 28 rewarded out of 30 responses. If all those cases in which a rat took 4 days to reach the criterion are pooled, some interesting regularities appear.

The 30 responses on each day were divided into six groups of five each. Then the average number of non-rewarded responses in each successive group of five was determined. The decrease in number of non-rewarded responses on any one day is only partly the result of extinction, since the other responses in each group of five were rewarded. Thus, the drop in each curve is the result of strengthening of the rewarded activity and also of the extinction of the non-rewarded activity. However, the rise in non-rewarded responses, from the end of one day's record to the beginning of the next, seems to be the result of recovery. This is the case since you recall that the retention curve for this kind of learning does not fall, but rises. These curves represent the complex resultant of both reward and non-reward (and undoubtedly other factors as well). If we take the information gained in studying the learning, extinction, recovery, and retention of responses to one bar alone and combine them for the two-bar situation, we should expect this sort of extinction and recovery, from one day to the next. And apparently it does occur.

THE TRANSPOSITION EXPERIMENT

There are other applications of conditioning principles to complex forms of rewarded behavior. One of these is in discrimination learning, especially concerning *relative* as contrasted with *absolute* responses to stimuli. The "transposition experiment" provides an example. An animal is presented with two stimulus cards (A and B), of different size, and trained to respond to B, the larger. Now, if it is presented with card B and a still larger card, C, the animal will make a majority of its responses to card C, even though it has never been rewarded for responding to C. Response to card C is a relative discrimination here, and response to card B would be an absolute one. This experimental finding has been used in Gestalt arguments against association theories, on the ground that, in relative responses, the animal is responding to a *relational* stimulus, an aspect of the total situation. The problem is complicated by the fact that, if card B is presented to the animal in conjunction with a much larger card, E, the animal will choose card B, an absolute response.

In a theoretical approach to the problem, Spence found that both relative and absolute choices could be accounted for, if the conditioning principles of positive and negative generalization were used. Later experimental findings have modified the shape of the generalization curves, but the relationships are still as originally presented by Spence.

The figures on the base-line in Spence's curves represent areas of stimulus cards in sq. mm. The two stimulus-cards, A and B, of the above example, have areas, we may say, of 160 and 256 sq. mm. respectively. In the initial training, the animal is rewarded when he responds to card 256, and not rewarded when he responds to card 160. When the animal is learning to respond to card 256, it is also acquiring a tendency to make the same positive response to other similar stimuli. The tendency diminishes as the stimuli differ in the size from card 256. This is the positive generalization curve. On the other hand, the animal is *not* rewarded when it responds to card 160. While it is learning not to respond to card 160, it is also acquiring a tendency *not* to respond to other similar stimuli as represented on the baseline. This tendency *not* to respond diminishes as the stimuli differ in size from card 160. This is the curve of negative generalization, or, as it is sometimes called, the generalization of inhibition. Now, let us suppose that the animal has been trained on this size discrimination, *i.e.*, he has a strong tendency to respond to card 256 and another, weaker tendency, not to respond to card 160. In order to predict his activity when we present him with a different pair of cards, we must consider the algebraic sum of his tendencies *to* respond and *not* to respond at each stimulus value. Using this theoretical structure, it is possible to predict the pairs of stimuli to which the animal will make an *absolute* response and those stimuli to which it will make a *relative* response. In addition to this, it was found that, with some pairs, the animal would make neither an absolute nor a relative response.

While this is a nice theoretical picture, the question is: Does it work out experimentally? The answer is: With certain qualifications, yes. With adjustment of the shape of the generalization curves, Spence has successfully applied this theory to discrimination learning in chimpanzees. Jackson and his students have performed a series of experiments on discrimination in children, aged 2½ to 6, in which these same principles were found to apply. It was further reported that the shape of the generalization curves is influenced by the amount of preliminary training, the spread of the curves being less, the greater the amount of training.

Trial-and-error learning and discrimination-learning are two of the better verified extensions of conditioning principles to more complex forms of behavior. I should now like to present some evidence that extinction and recovery also occur in rewarded verbal learning in humans.

REWARDED VERBAL LEARNING

It has been found by Thorndike, Lorge, Rock, and others, that, when a subject is presented with a list of words and asked to give a number between one and eight for each of the words, the subject learns those word-number connections which are called "right" by the experimenter.

However, in the same situation, if a number-response is called "wrong" by the experimenter, the subject does *not avoid* it, and even makes the same number-response, on later trials, with better than chance frequency. This startling finding has been repeatedly confirmed.

It will be noted that the number-responses, which were not avoided upon being called "wrong," were probably not connected with the particular word by any strong habit structure. The subject would have had little opportunity or need to associate any particular number between one and eight with the words in the experiment.

This raises the question of what would happen to a response that was already learned, if it were repeatedly called "wrong" by the experimenter.

Twenty-one college students served as subjects. Reading from a forty-word list, the experimenter said a word, and the subject responded with any number between one and eight. The experimenter then said "right" or "wrong" according to the number's predetermined correctness. This list of forty words was repeated eight times in this manner. Altogether, each subject was given eight trials on each of three different lists. Each subject was then dismissed and told to return two days later at the same time.

The experimenter then noted all the word-number connections which conformed to the following criteria:

1st—The connection had been correctly made 3, 4, 5, or 6 times during the eight trials, with the added provision that only those connections were chosen which had occurred correctly in the last three of the eight trials.

2nd—Connections occurring in the first four words or in the last four words of a list were not used.

3rd—No connections were used which had been made correctly on the first trial. This was done in order to rule out, as far as possible, associations already established in the subject by previous experience.

In the records for the 21 subjects, there were 147 word-number connections which fulfilled these requirements. *These 147 connections were divided into two groups* with approximately equal numbers of the 3, 4, 5, and 6-time correct responses, in each group. Seventy-one connections were assigned to the "Reward" group and 76 to the "Punishment" group.

Two days after learning, a second series of eight trials was given. The procedure was substantially as before and the subjects were not told that anything would be changed. The "reward" group was treated exactly as before. However, the responses of the "punishment" group were called "wrong." All other number-responses were treated as before.

Twenty-four hours after this second series, another series of five trials was run, in which the procedure was exactly the same as in the second. This third series was run in order to test for recovery between the last trial of the second series and the first trial of the third. This was on the punished connections, of course.

The changes that interest us are those that occurred during the second and third series. On the first trial of the second series, the subjects show that they have retained about 58% of the connections learned two days before. Those that are rewarded by "right" are retained and the subjects then proceed to relearn many of those that had been forgotten. *After* the second series, some are again forgotten and partially relearned in the third series. The connections that are "punished" drop from 58% to 16%, showing a sharp drop in the early punishment, or extinction, trials. The drop in this case is 42 per cent points.

During the 24 hours between the second and third series, the punished responses rise in correctness by 14 per cent units. This rise has over 98 chances in 100 of being a true one and is shown by all of the various groups of connections that were correct 3, 4, 5, and 6 times during the eight learning trials. Moreover, on the 8th trial, the punished responses have dropped only to 16%, so that the subsequent rise cannot be attributed to a return to a chance level of responding. Chance in this case is 12½%. *Apparently, recovery is a characteristic of this sort of learned-and-then-punished connections.*

It is also interesting to examine the relative resistance to extinction of the connections rewarded 5 and 6 times and those rewarded 3 and 4 times. Although the 5 and 6 reward curve is not much higher than the 3 and 4 curve, the direction of the difference is maintained in 10 of the 13 possible instances. The amount of original training seems to affect the resistance to extinction, a finding similar to that found in studies of the lever-pressing response in rats. A greater irregularity of the 3 and 4 curve may be the result of the smaller N, or it may be that with a smaller number of original rewards, the connections do not respond to punishment in the same way. The latter possibility seems more likely, since Thorndike and others have found that with *no* original learning, verbal punishment has no decremental effect.

Calling a word-number connection "wrong," and lack of reward when a rat presses a bar, are certainly not the same, but they may be analogous. When a rat has been receiving food following depression of a lever, and then, suddenly, no food appears after a response, there is certainly an objective lack of reward. We do not know what the rat "thinks" about it, of course. In word-number studies on college students, however, if the number response is followed by neither "right" nor "wrong" but only by an uninformative word such as "next," the situation is different. Questioning the subjects reveals that they are just as likely to believe they were right, although a number of them say they do not know what it means. The word "next," following a number-response, certainly gives no objective indication of success or failure.

In order to determine the effect, if any, of the word "next" on previously rewarded word-number connections, an experiment was run with the aid of Miss Jean Ackermann of Barnard. The procedure was the same as before, except that, on the second day, the word "next" was substituted after 19 word-number connections. Only those connections were used, which had been correct 5 or 6 times during the first day's training. When "next" is used, no great change occurs in the number of correct responses. There is a slight drop in the retention curve during a 24 hour retention-interval, but not greater than would be expected on verbal material that was learned 48 hours before. In comparing these results with those for lever-pressing in rats, it is probably true to say that there is nothing in the rat situation which is analogous to "next" in the word-number experiments. Possibly, however, punishment in the verbal situation is analogous to extinction for the rats.

One of the other principal points of difference between the rat lever-pressing and the word-number connection, is that, with word-number connections, the retention curve falls, as in retention of other verbal material. If we assume the interference theory of forgetting, the difference seems a little clearer. After college students have learned word-number connections, they continue to use words and numbers during the retention interval. There is interference, or "retroactive inhibition," and "forgetting" occurs. On the other hand, the rats live in their cages during the retention interval and have little opportunity to manipulate levers. This might account for the lack of drop, but does not explain the rise in the rat's retention-curve, which has to be interpreted in the light of other principles.

Other Applications

In this discussion, I have not as yet mentioned the greatest single application of conditioning principles to the field of verbal learning. This is the "Mathematico-Deductive Theory of Rote Learning" by Hull, Hovland, Ross, Hall, Perkins, and Fitch. It did not seem possible to compress it into ten minutes, or even fifteen minutes. This work began with an assumption which is easily stated. It is as follows: *In rote learning, the remote associative bonds of Ebbinghaus have the characteristics of the Pavlovian trace conditioned response.* With this as a starting point, and using a large number of other principles as well, Hull has presented a series of progressive approximations toward an adequate statement of the principles of rote learning. The "Mathematico-Deductive Theory" is the latest approximation, more adequate because thoroughly quantitative, but still, as he points out, an approximation. It is one of the most complete, if not *the* most complete, statements of assumptions, inferences, and experimental relationships in any field of psychology today.

In the field of concept-formation, there are at present only suggestions. Humphreys has introduced a kind of conditioning experiment which he calls "Expectancy learning." It is also describable as a very simple form of concept formation and it also shows extinction and spontaneous recovery.

In problem-solving, we have heard since Ruger's experiments and before, that problem-solving is "mental trial and error." It is possible that the various "hypotheses" that occur to the problem-solver may appear in an *order* which is like that found in trial-and-error learning in rats or in theater-ticket behavior in men. In a recent conversation, Keller reported that human subjects, responding in a modified Skinner-box situation, show a change in hypothesis as a change in the slope of the curve of cumulative responses. Interestingly, the slope sometimes changes *a little before* the subject can verbalize the new hypothesis. There are other experiments now in progress which are attempting to control the appearance of hypotheses in problem-solving by subjecting them to strengthening and extinction procedures.

In this report, the goal has been to show some of the representative areas in which conditioning principles have been found useful. The conditioned reflex itself is now seen to be only a very minor aspect of learning, if indeed it is a kind of learning. The principles, which have spread so widely, are themselves frequently questioned. Extinction may soon be considered as a special case of learning, learning of an alternative incompatible response. Spontaneous recovery would then be merely another retention curve, and secondary extinction would be a kind of positive generalization or transfer. Generalization, itself, occurs along stimulus-dimensions which may possibly be more adequately approached by psychophysics. However, in terms of present formulations, conditioning principles have brought light and explanatory power to a fairly wide range of psychological problems.

SECTION OF ANTHROPOLOGY

February 23, 1942

Doctor Cora Du Bois, Sarah Lawrence College, Bronxville, N. Y. and Doctor Emil Oberholzer, New York, N. Y.: *Rorschach Tests and Native Personality in Alor, Dutch East Indies.*

This study is an attempt to apply Rorschach's experiment cross-culturally. The conclusions are based on the fact that the Alorese Rorschach findings are completely different from those of normal Europeans and Americans. The study was confronted with this difficulty from the beginning. The approach to some of the fundamental features in Alorese psychology was possible only because of experience with Rorschach tests in psychopathology.

The test results of the Alorese differ in almost every experimental factor from those of Americans and Europeans. In addition, the Alorese exhibit specific reactions which are missing in normal Europeans and Americans. Apart from the Alorese, these reactions occur only in certain pathological states of mind. However, they never occur to the same extent as among the Alorese, where they are part and parcel of their reactions to the non-colored cards of Rorschach's ten ink blots. They come nearest to the findings in the defective states of the traumatic constitution. This offers the clue to an understanding of the specific reactions in the findings of the Alorese.[1]

The Alorese Rorschachs show no movement answers. These answers indicate that the picture or parts of it are seen in movement. Movement answers, in conjunction with color responses, show the stability of a person's emotions to the point of complete self-control and balance of mind. In addition, the proportion of the various color reactions are completely reversed in Alorese and European tests. The form colors represent emotional responsiveness and adjustment. The color forms and the primary colors (such as blood, water, etc.) are the expression of egocentric affectivity and impulsiveness which suffer no control and adaptation. The form color, which prevails in findings of normal Europeans and Americans and which constitutes nearly one half of all the color reactions in European material, is missing in the Alorese. In its place, occur the color form and, still more, the color responses. These do not occur in normal Europeans whereas they amount to almost one half of the color reactions of the Alorese. That is, their color form and color about equal the amount of European form color. The singularity of the Alorese findings, therefore, is best evidenced by the fact that they would represent pathological conditions in Europeans and Americans.

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[1] Oberholzer, E. Zur Differentialdiagnose psychotischer Folgezustände nach Schädeltraumen mittels des Rorschachschen Formdeuterversuchs. Zeitschrift f. d. ges. Neur. und Psychiatrie, 1931. See also "Report on Rorschach's Experiment in Traumatic Mental Disorders" at the first International Neurological Congress in Berne, 1931.

The conclusions to be inferred are that the personality and emotional character of the Alorese are very different from normal Europeans and Americans. The Alorese are fearful and distrustful. Measured by our responsiveness, they lack inter-individual contact and rapport, and possess an excess of emotional reactions which occurs only in American and European psychotics or serious neurotics. Last, but not least, the Alorese have an attitude of passive acceptance. They differ from Europeans and Americans in respect to the degree and kind of intelligence. As to its degree, they rank on the level of our morons, or, at the best, of our most unintelligent. The intelligence of most of the Alorese is equivalent to that of the feeble minded among Americans and Europeans. Nevertheless they are neither morons nor feeble minded, just as they are not schizophrenics or traumatics. Their Rorschachs and psychology are something *sui generis*. Among themselves, in their culture and milieu, they are adjusted and their Rorschachs represent their norm.

From the tests, it is evident that, within the range of the characteristic personality structure, there are wide individual differences. The individual personality of the Alorese can be inferred from the varying number of color reactions just as it can be for an American or European. The latitude of their variations is even considerably greater than in Europeans and Americans.

There is only one quantity among the numerical values of the experimental factors that the Alorese have in common with Europeans. The form answers, i.e., those interpretations which are determined by the form and outlines of the pictures or their parts, are not only the majority of all the answers, but occur in the same percentage among the Alorese and Europeans. It remains to be seen whether other natives, as different from the Alorese as the Alorese are different from the Europeans, exhibit the same striking fact.

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 27 AND 28, 1942

Conference on "*Physics of the Solid State.*"

The Section of Physics and Chemistry held a Conference on "Physics of the Solid State," as the third in the series for the Academic Year of 1941–1942. Professor P. Debye, Cornell University, Ithaca, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by P. Debye.

"Co-operative Phenomena in Solids," by J. G. Kirkwood, Cornell University.

"Thermal Disorder in Crystal Lattices," by H. W. Zachariasen, University of Chicago.

Structure and Degree of Order in Glass," by B. E. Warren, Massachusetts Institute of Technology.

"Structure and Order in Macromolecular Solids," by W. O. Baker and C. S. Fuller, Bell Telephone Laboratories.

"The Plasticity of Crystals and Lattice Imperfections," by Frederick Seitz, University of Pennsylvania.

"Paramagnetic Relaxation," by J. H. Van Vleck; Harvard University.

"Recent Progress in the Study of Co-operative Phenomena," by G. H. Wannier, University of Iowa.

"Molecular Rotation in Some Mixtures of Crystalline Organic Solids," by S. O. Morgan, Bell Telephone Laboratories.

"Absorption of Elastic Waves in Solids," by H. S. Sack, Cornell University.

ANNOUNCEMENT OF PUBLICATIONS FOR 1942[1]

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution, except as qualified in the footnotes:[2]

ANNALS:

1. "Thermodynamic Properties of Solutions of Long-Chain Compounds," by Maurice L. Huggins. (Approximately 35 pages.)
2. "Immunochemistry." Papers delivered at the conference by this title. (Approximately 100 pages.)
3. "Studies on Fresh Water Bryozoa, XII. A Collection from Various Sources," by Mary D. Rogick and Claudeous J. D. Brown. (Approximately 30–40 pages.)
4. "An Analysis of the Locomotion of the Sea Horse (*Hippocampus*) by Means of High Speed Cinematography," by Charles M. Breder, Jr. and H. E. Edgerton. (Approximately 50 pages.)
5. "The Ultracentrifuge." Papers delivered at the conference by this title. (Approximately 100 pages.)
6. "Protein Hormones of the Pituitary Body." Papers delivered at the conference by this title. (Approximately 100 pages.)

TRANSACTIONS:

Series II, Volume 4, Nos. 1–8. These papers are sent to all members of the Academy, regularly, during the academic year. (Approximately 200–250 pages.)

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[1] Notice of additional publications to be added to this list will be sent to members later.

[2] Active, Sustaining, Life, and Honorary Members may receive, upon request, a copy of all current numbers of the Annals.

Student and Associate Members are entitled to receive one complete monograph or up to 150 pages of smaller papers.

SCIENTIFIC SURVEY OF PORTO RICO AND THE VIRGIN ISLANDS:[3]

Volume XIX, Parts 1 and 2: "Meteorology of Porto Rico," by Robert G. Stone.

Volume XIX, Part 3: "Meteorology of the Virgin Islands," by Robert G. Stone.

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[3] The Scientific Survey publications are available to Members for $1.50 per part and to non-members for $2.00 per part.

A certain number of parts, as announced each year, are available free to Honorary, Life and Sustaining Members.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 4 APRIL, 1942 No. 6

SECTION OF GEOLOGY AND MINERALOGY

MARCH 2, 1942

DOCTOR FRANK M. SWARTZ, The Pennsylvania State College, State College, Pa.: *Silurian and Early Devonian Studies in the Middle Appalachians.*[1] (This lecture was illustrated by lantern slides.)

Historically, the modern knowledge of Appalachian Silurian deposits and faunas began with the work of James Hall in western New York. A further great advance was the elaboration of the sequence of these strata in western Maryland, accomplished by 1923 through the labors of C. K. Swartz, W. F. Prouty, E. O. Ulrich, G. W. Stose, and others.

During subsequent years, it has been the writer's privilege to be associated with C. K. Swartz in investigations, still largely unpublished, of the Silurian and, to some extent, the early Devonian from Maryland through the Virginias into northeasternmost Tennessee and also through eastern Pennsylvania into southeastern New York. Certain results of these investigations will be drawn upon along with those from separate work of the writer in

[1] Published by permission of the Pennsylvania Geological Survey.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 4, No. 6, April, 1942.

This publication is distributed to Members and is published monthly from November to June, inclusive, at 109 West Chestnut Street, Lancaster, Pa., by The New York Academy of Sciences, Seventy-ninth Street and Central Park West, New York City.

Associate Editor: Eunice Thomas Miner, Executive Secretary.

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eastern and especially central Pennsylvania, which has received the support of the Pennsylvania Geological Survey. In all the areas, numerous local sections have been measured and worked out in detail to provide an objective basis for understanding the deposits.

The present paper is devoted to the study of the Silurian and Lower Devonian formations of the northern part of the middle Appalachian region, embracing Maryland, Pennsylvania, New Jersey and southeastern New York.

Near Cumberland, western Maryland, the early Devonian, Silurian, and latest Ordovician include the following formations, given in descending order:

Lower Devonian:
  Oriskany group:
    Ridgeley sandstone, 130 feet
    Shriver chert, 115 feet

  Helderberg group:
    Mandata shale, 20 feet
    New Scotland limestone, 30 feet
    Coeymans limestone, 10 feet

Silurian:
  Keyser limestone, 280 feet (Silurian or Devonian)
  Tonoloway limestone, 600 feet
  Wills Creek shale, 500 feet
  Bloomsburg beds, 20 feet
  McKenzie shale and limestone, 300 feet

  Clinton group:
    Rochester shale, 30 feet
    Keefer sandstone, 12 feet
    Rose Hill shale, 600 feet
  Tuscarora sandstone, 280 feet

Ordovician:
  At top, Juniata red beds, 1000 feet, included in Silurian by some authors.

The formations recognized in western Maryland have proven to be of wide extent through the middle Appalachian region. They nevertheless undergo striking lateral changes which will be briefly outlined.

The middle Appalachian Silurian beds reach their greatest thickness toward eastern Pennsylvania, where they become over 4,000 feet thick. In the latter area, they are formed primarily of Shawangunk conglomerate and sandstone below, and Bloomsburg red sandstone and shales above, together with a subordinate thickness at the top of Poxono Island shale, Bossardville limestone, and Keyser sandstone and limestone. As shown by interfingering of the deposits, the Shawangunk is an easterly phase which corresponds essentially in age to and laterally replaces the Tuscarora sandstone, Clinton shales, and apparently the lower part of the McKenzie beds of the more westerly area. The greatly thickened easterly Bloomsburg red beds similarly correspond to and replace the upper part of the McKenzie, together with the Wills Creek shale and probably some of the lower Tonoloway of western Maryland and central Pennsylvania. The Bossardville limestone at Schuylkill Haven, eastern Pennsylvania, contains ostracodes characteristic of the uppermost Tonoloway in parts of central Pennsylvania, while the Poxono Island shale presumably represents some of the middle part of the Tonoloway. The limits of the Keyser or Decker to Manlius beds of eastern Pennsylvania correspond rather closely to those of the Keyser of western Maryland.

Continuing northward from eastern Pennsylvania to the Helderberg escarpment of eastern New York, all but the topmost Silurian beds drop out rapidly by unconformity.

The Silurian beds thin southward from Maryland to Tennessee, with eventual disappearance by unconformity of the Keyser limestone and also of some of the median formations of the Silurian. As in Pennsylvania, the more easterly exposures tend to show replacement of marine by non-marine sediments, but the thicknesses are less.

The marine portions of the Silurian sequence are characterized by well defined and comparatively widespread faunal zones. In part, these are based on brachiopods, but ostracodes have been especially valuable. The most important of the Silurian zones of the area are as follows:

Keyser limestone:

Zone of *Favosites praecedens* and *Meristella praenuntia*, with subzones of *Rensselaeria mutabilis*, *Tentaculites gyracanthus*, and *Spirifer vanuxemi;* also, in certain areas, with relatively persistent stromatoporoid and coral beds, the corals including *Cladopora rectilineata.*

Zone of *Chonetes jerseyensis*, with subzones of *Merista typa*, *Calymene camerata*, *Gypidula prognostica*, *Stenochisma deckerensis* and *Reticularia modesta. Cladopora rectilineata* occurs in some coral beds of rather wide extent. At base in central Pennsylvania is a persistent zone of *Leperditia scalaris.*

Tonoloway limestone:

At middle a widespread zone of *Hindella congregata*, overlapping in its lower part a zone of *Spirifer vanuxemi.* At top of Tonoloway in central Pennsylvania and in Bossardville at Schuylkill Haven is a zone of *Kloedenia centricornis* var. with the median lobe surmounted by a slender spine. *Welleria obliqua* is rather widespread in about the lower half of the formation. Other Ostracoda will also prove of zonal value.

Wills Creek shale:

Sparingly fossiliferous; locally in Maryland with a zone of *Spirifer vanuxemi* near middle; near base where Bloomsburg is thin occurs *Zygobeyrichia ventricornis.*

Bloomsburg red beds.

McKenzie shale and limestone:

Camarotoechia andrewsi and *Homeospira marylandica* are abundant in middle and upper parts; *Reticularia bicostata* is abundant in the *Beyrichia moodeyi* zone and often at lower levels as well; *Orthoceras mackenzicum* occurs near the middle. The more restricted zones are as follows:

- *Kloedenella gibberosa* zone
- *Kloedenella nitida* zone
- *Beyrichia moodeyi* zone, with recurrent *Kloedenia ventralis*
- Barren beds
- *Kloedenia ventralis* with profuse *Whitfieldella marylandica*
- *Schuchertella elegans*—*Kloedenella cornuta* zone, with important Rochester shale elements, perhaps better classed as Rochester shale

Clinton group:

- Rochester shale:
  - *Drepanellina clarki*—*Dalmanites limulurus* zone
- Keefer sandstone:
  - *Schuchertella subplana* fauna in lower part in central Pennsylvania
- Rose Hill shale:
  - *Anoplotheca hemisphaerica* below, replaced near top by *A. sulcata; Liocalymene clintoni* in middle and upper parts. *A. sulcata, L. clintoni, Chonetes* near *C. novascoticus* disappear abruptly at top. More restricted zones are as follows:
    - *Mastigobolbina typus* zone
    - *Bonnemaia rudis* zone
    - *Mastigobolbina modesta* zone, with *Zygosella postica*
    - *Mastigobolbina lata* zone
    - *Zygobolbina emaciata* zone
    - *Zygobolba bimuralis* zone
    - *Zygobolba decora* zone
    - *Zygobolba anticostiensis* zone
- Castanea sandstone, in central Pennsylvania
- Tuscarora sandstone:
  - With horizons of *Arthrophycus harlani* and of some eurypterids

Of the above zones, the greater number of those of the Keyser and Tonoloway were first established in Maryland by C. K. Swartz, and those of the Clinton by E. O. Ulrich and R. S. Basslers. Work on the McKenzie zonal ostracodes led to the writer's interesting discovery of the characteristic terminal dimorphic swellings of the Silurian Kloedenellidae.

The zones of *Spirifer vanuxemi* of the middle Wills Creek, middle Tonoloway, and upper Keyser represent recurrences controlled fundamentally by environmental relationships, and environmental factors affect all of the faunal assemblages. However, most of the zonation, and especially that of the ostracodes, involves primarily the evolutionary development and extinction of species and genera. Some of the zonal boundaries are particularly abrupt and may represent appreciable withdrawals of the sea. The top of the Rose Hill shale, for instance, is marked by the sudden disappearance, generally within one or two feet or less, of *Chonetes* near *C. novascoticus*, *Anoplotheca sulcata*, *Liocalymene clintoni*, *Mastigobolbina typus*, *M. triplicata*, *M. punctatus*, *Plethobolbina typicalis*, *Bonnemaia celsa*, *B. crassa*, *Zygosella vallata*, and *Dizygopleura loculata*. The extinction of the ostracode species involves the termination of the genera *Mastigobolbina*, *Bonnemaia*, and *Zygosella*, which provide many zonal species in the Rose Hill. An equally abrupt but less sweeping faunal change occurs at the top of the *Drepanellina clarki* zone, and the writer has been using this faunal boundary for the Rochester-McKenzie contact. A further change, not quite so sharply defined as seen in the field, but involving the disappearance of the last important Rochester shale faunal elements, takes place at the top of the *Schuchertella elegans-Kloedenella cornuta* zone.

The depositional relations of the *Schuchertella elegans-Kloedenella cornuta* zone present features of much interest. This zone is never more than 10 feet thick at localities where it has been measured. In view of this small thickness, together with the apparent, though evidently not long continued breaks, represented by the marked faunal changes at the base and top, it is surprising to find the zone persistently developed at section after section from Danville and Williamsport, in northcentral Pennsylvania, southward into Virginia. Through this area of 10,000 to 15,000 square miles, the zone is not missing at any studied locality where the normal sequence across it is exposed for examination. The sediments of the zone are persistently calcareous shales with interbedded thin layers of limestone, the lower limestones tending to contain some fine quartz sand. Toward the east, the zone may be represented by more sandy phases, beyond the area where the fauna has been found. Toward the west, the zone disappears below younger beds at the Allegheny Front, and its actual western limits are unknown.

The next overlying zone, characterized by *Kloedenia ventralis* together with very profuse *Whitfieldella marylandica,* is coextensive with the *Schuchertella elegans* zone, and is comparable thereto in thickness and in geographic and lithologic persistence. Its upper limits intergrade with the next overlying generally barren beds, and the *Kloedenia* recurs in the still higher *Beyrichia moodeyi* zone.

Many of the other Silurian zones have greater geographic distribution, but, in general, are not quite so limited stratigraphically, and do not have such sharply marked, apparently disconformable boundaries.

The facies relations of the faunas of the area under consideration are of interest. The Tuscarora sandstone contains rather numerous horizons of the annelid burrows named *Arthrophycus harlani,* best seen as casts on under surfaces of sand beds deposited on burrow-marked clay interbeds. Some eurypterid "chowder" beds have been found in Pennsylvania to the westernmost belt of Tuscarora crop. Shelly faunas are notably absent. Similar faunal relations characterize the Shawangunk, including beds of Clinton as well as Tuscarora age.

The Rose Hill shales contain brachiopod-bearing beds often rich in individuals, but generally few in species except in limy beds near the top of the formation. Ostracodes are frequently profuse. Trilobites are moderately diversified. There are some pelecypods. Gastropods are few, although the pteropod-like

Tentaculites minutus is abundant on occasional bedding surfaces. Nautiloides have not been observed. Well preserved pelmatozoan cups are not found, although stem plates are common in some beds. The button-coral, *Paleocyclas rotuloides*, is locally common near the top, but otherwise corals are very rare, as are bryozoa. Conspicuously absent are the graptolites. Only one graptolite rhabdosome has been seen by the writer from the Rose Hill shale, and none from other Silurian beds of the Pennsylvania-Virginia region, although *Monograptus* is common in some Clinton beds in western New York.

The *Schuchertella subplana* fauna found in the Keefer sandstone in central Pennsylvana is relatively diversified, except for an interesting paucity of ostracodes. Ostracodes are again profuse in the *Drepanellina clarki* zone along with brachiopods and trilobites, and rarer bryozoa, pelecypods, and gastropods. The McKenzie formation is notable for limestone layers with profuse ostracodes, along with others with none, often suggesting current-concentrations of the little shells. Brachiopods are individually profuse in a good many beds but are generally rare in species. Pelecypods are locally abundant but as a rule poorly preserved. Near the middle of the formation, the gastropod *Hormotoma hopkinsi* abounds in occasional layers, and the cephalopod *Orthoceras mackenzicum* is common in a thin zone. Trilobites are almost wholly absent, except for a few in the *Schuchertella elegans* zone, and several specimens found in the upper McKenzie in Maryland. There are a few bryozoa. Occasional heads of the honeycomb coral, *Favosites*, are to be observed, apparently representing the Lockport species, *F. niagarensis*. Otherwise, corals are virtually absent, except in certain beds interfingering with the *Beyrichia moodeyi* zone near Lock Haven in northcentral Pennsylvania, and containing in profusion the Lockport *Cladopora multipora*. It is regrettable that these coralline beds are present only in a small area at the margin of the belt of Silurian outcrop, and that, farther northwest, they are buried below younger sediments and so can not be studied to obtain more critical evidence for or against the view that they represent a tongue from the Lockport limestone of western New York. It is of interest that, like the Lockport, the actual coral-bearing beds at Lock Haven are themselves devoid, or nearly so, of the characteristic McKenzie ostracodes.

The Bloomsburg red beds lack known faunas, except for rare *Leperditias* in westward marginal phases. Near Loysville, Pennsylvania, a 20-foot red sandstone near the top of the Wills Creek, and believed to represent a tongue from the higher part of the more easterly, thicker Bloomsburg, contains numerous specimens of the ostracoderm fish, *Paleaspis americana.* However, these are not known to occur in the Bloomsburg proper.

The Wills Creek shales are rather sparingly fossiliferous, most of the beds being barren, or, at times, containing rather poorly preserved *Leperditias.* Fossils are common in some parts of the Tonoloway in Maryland and the Virginias, and the faunas there include elements which were thought at one time to suggest approximate equivalence of the Tonoloway to the Cobleskill to Manlius beds of central New York. Traced northward through central Pennsylvania, however, the Tonoloway becomes increasing depauperate, with loss of any faunal suggestion of relationship with the Cobleskill-Manlius deposits.

Deposition of the Keyser limestone brought with it a widespread return of profuse and diversified marine stromatoporoid, coralline, bryozoan, and brachiopod faunas. Crinoidal limestones are locally developed. Ostracodes are, at times, common, and trilobites occur. Northward persistence in the lower Keyser of the Cobleskill-occurring *Halysites aff. catenularia, Chonetes jerseyensis*, and *Calymene camerata*, as compared to disappearance of the marine assemblages in the Tonoloway, strongly supports the view that the lower Keyser is continuous with the Cobleskill of central New York; as does the close similarity and probable identity of the lower Keyser *Reticularia modesta* and variety *plicatus*, on the one hand, and the Cobleskill *R. corallinensis* on the other. The upper Keyser *Spirifer vanuxemi-Tentaculites gyracanthus* faunas are likewise suggestive of the New York-Manlius limestone.

In the sense that the Cobleskill to Manlius beds of central

New York have long been classed as Silurian, the Keyser can also be so regarded. Actual correlation to Silurian and non-equivalence with earliest Devonian deposits of England and Europe are by no means established, and, in this sense, classification of the Keyser remains an open question. Diastrophically and paleogeographically, the Keyser represents the return of open seas to the middle Appalachian region, following a period of more restricted oceanic connections during Wills Creek and Tonoloway deposition; thus inaugurating conditions which continued through the Helderberg and Oriskany of the Lower Devonian.

The lower as well as the upper boundary of the Appalachian Silurian has been subject to discussion. Relations of the Tuscarora to the underlying Juniata have been considered in central Pennsylvania, with investigation of numerous sections and the attempt to recognize and correlate subdivisions of the Juniata deposits. Whatever else is true, it is clear that the Tuscarora—Shawangunk beds widely transgress the Juniata deposits, and that marked geographic changes occurred from Juniata to Tuscarora time. On the other hand, extensive occurrence of pebble beds at the base of the Juniata in central Pennsylvania suggests that the acme of Taconic uplift was then attained. The diastrophic history is complex, and, in the writer's opinion, is of questionable value, except in a very general way, as a criterion for correlation in terms of the Siluro-Ordovician boundary as seen in England.

Turning briefly to the Lower Devonian, the *Gypidula coeymanensis* and *Eospirifer macropleurus* faunas of the Helderbergian Coeymans and New Scotland limestones, and the *Spirifer arenosus* fauna of the Oriskany proper, are widespread through the middle Appalachian region, furnishing excellent correlations of the deposits in which they are found. Relationships of the Shriver chert, lying between the *macropleurus*- and *arenosus*-bearing beds in western Maryland and central Pennsylvania, have been more troublesome; but are now being clarified by studies of Lower Devonian ostracodes, especially those of the family Thlipsuridae. The ostracode assemblage of the Shriver of western Maryland has been duplicated in collections provided by Chadwick from the lower part of the Oriskanian Glenerie limestone at Alsen, New York; but has not been discovered in central Pennsylvania, where the middle to upper Shriver, as there seen, contains a related but younger ostracode fauna, close to that of the lower Glenerie at Kingston, New York. The Port Ewen at Kingston, New York, and at the Delaware Water Gap, Pennsylvania, contains a third related but distinct set of ostracodes, older than that of the Shriver of Maryland, and this assemblage also occurs in Mandata beds in Perry County, Pennsylvania. Additional and distinct ostracode assemblages have been obtained from Glenerie beds near Catskill, New York, and from questionable Mandata beds at Warren Point, Pennsylvania. The facts that the western Maryland Shriver ostracodes occur in the Glenerie at Alsen, and that the Port Ewen species persist into the Mandata of Perry County, do much to show that these assemblages have zonal time value, and that they are not simply provincial in significance, as seemed possible at an earlier stage of their study. Ostracodes have also been obtained from the Middle Devonian Onondaga beds of central Pennsylvania and northeastern West Virginia, and again show marked zonal values.

Work on the ostracodes of the Shriver of central Pennsylvania has led to discovery of fine trilobites and conularids, providing some interesting information on several of their structural aspects, especially in the case of the Lower Devonian dalmanitid trilobites.

In summary, the Silurian deposits of the middle Appalachian region comprise a sequence of dominantly clastic materials, which reach a maximum thickness of about 4000 feet. The clastics become coarser and in considerable part continental toward the east; especially toward eastern Pennsylvania where, for the most part, they are formed by the Shawangunk conglomerate below, and Bloomsburg red beds above. By these changes, the clastics show clearly their derivation from the easterly oldland Appalachia.

The sedimentary facies necessarily show varying geographic distributions; but these are variations within deposits of, at any one time, one geographically continuous basin of deposition, and there is no known evidence within the area of study that the basin was at any time subdivided by arches such as were developed in some parts of the Appalachian region during Ordovician time.

There are significant changes in thicknesses of the sediments. Thus, the marked thickening of the clastics toward eastern Pennsylvania strongly suggests that they were there fed by a major river system or systems issuing westward from the well watered source land. Presumably, the Bloomsburg was in large part deposited directly by fluvial waters. Portions of the Shawangunk—Tuscarora materials may have been spread in shallow brackish waters, but, in any event, lack the shelly faunas which might be expected had the shallow waters been truly marine.

The thickness changes are significant but not rapid. Maximum convergence angles are about one degree. Commonly, the angles of convergence of upper and lower surfaces of sediments of one age are to be measured in a few minutes or seconds of arc or less. Such rate of thickness changes of the suspension-borne sands and clays argues that the currents were relatively active and were, in general, spreading the materials over a comparatively flat platform, mostly in shallow marine waters, but, in part, in lagoonal and alluvial areas. The deposits of the *Schuchertella elegans* zone appear to be an extreme example of deposition over a flat platform covered by a shallow sea inundating an extensive area, during a geologically brief time period.

The finely marked faunal zones, in general, involved either rapid evolutionary change of the concerned faunal elements, or slowness of deposition of the sediments, or, very likely, both. Comparatively wide distribution of the clastics would tend to thin them at any one place.

The faunas show clear environmental relationships, and, in general, the shelly assemblages are such as would live in shallow reaches of the sea, mostly with the waters not continuously clear, and perhaps a little brackish. Through most of Silurian time coralline faunas are inconspicuous, though they were comparatively prominent during early to middle parts of the period toward Indiana, Illinois, and Ontario. They are but sparsely and locally present in the McKenzie, and are extensive only in the Keyser limestone. The planktonic graptolites are notable for their absence, and nautiloides are common at only a few horizons. The facies relations of the faunas must be given due regard in considering correlations with other areas.

The distributions and relations of the sediments and faunas of the area of study are thus quite unlike those pictured by Jones for the Silurian of England, where he envisions a moderately deep geosynclinal area with thin axial clays, thickening into coarser, poorly washed, submarginal, clayey sands, and with thin and discontinuous well washed sands, clays, and limes forming in shallow shelf waters; the shelf areas inhabited by shelly faunas, giving place to graptolites toward the axis of the geosyncline. Perhaps it would be better to say that the marine beds of the Silurian of the middle Appalachian area of crop are essentially comparable to the shelf-sea deposits of the English area, and that the prime differences rest in the far wider distribution, greater continuity, and greater thicknesses of this facies, combined with lack of the deeper water sediments, and the presence in Pennsylvania, especially, of a great body of continental red beds which find no counterpart within the English Silurian.

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SECTION OF BIOLOGY

MARCH 9, 1942

DOCTOR EARL H. MYERS, Woods Hole Oceanographic Institute, Woods Hole, Massachusetts: *Biology of the Foraminifera and Their Significance in Paleoecology.* (This lecture was illustrated with lantern slides and a motion picture on the life cycle of *Patellina corrugata* Will.)

Foraminifera have probably inhabited all oceans capable of supporting life. This is evident from the universal distribution of the tests in marine sediments both recent and geologic. The extent of their distribution can be appreciated when we recall that approximately 70% of the earth's surface is covered by the sea and that 70% of all land areas are overlaid by rocks of marine origin. Therefore, approximately 90% of the surface of the earth either is or has been the natural habitat of these marine rhizopods. This is true because these organisms live at all depths on the floor of the sea and in the overlying water.

Globigerina ooze, consisting largely of the tests of pelagic species now forming on the floor of the sea, covers an area that is approximately equal to 25% of the earth's surface. Little is known at this time concerning how numerous these organisms are in the sea, the rate at which they are contributed to the sediments, or the sequence of events in the life history. It has been presumed that these species are pelagic at all times, but the fact that many of those most abundantly represented in *Globigerina* ooze are seldom taken in plankton hauls, suggests otherwise.

Recent studies on the life cycle of *Tretomphalus bulloides* show how a typically benthonic or bottom dwelling foraminifer may develop a pelagic phase. From cultures, we have determined that *Tretomphalus* gives rise to sexual gametes which unite in fertilization to produce a zygotic amoebula. This develops into the multinucleate microspheric phase of the life cycle often referred to as *Discorbis isabellina*. *D. isabellina* asexually produces mononucleate megalospheric individuals that are commonly referred to as *D. concinnus*. Upon reaching the growth limit for this phase of the life cycle, the individual develops a large gas-filled float enclosed within a float chamber. This Tretomphalus stage rises to the surface of the sea where it extends filose pseudopodia in all directions beneath the surface film of the water. Should the pseudopodia of two individuals come into contact, they anastomose, thicken, and draw the two tests together, in such a manner that the gametes, streaming through large openings in the tests, which are now approximately opposed, have a maximum opportunity to unite in fertilization. This is perhaps one of the most remarkable adaptations of a species to assure the fertilization of the gametes and the dispersal of sexual products to be found in the animal kingdom. The genus *Tretomphalus* belongs to the family *Cymbaloporettidae*, while *Discorbis* belongs to the family *Rotaliidae*. Therefore, this work conclusively demonstrates that the three species of the two genera mentioned are actually different phases in a single life cycle.

A second method of association of sexual individuals, which assures a maximum opportunity for the fertilization of the gametes, was observed in several other species of *Discorbis*. In *D. opercularis*, two or more mononucleate megalospheric individuals associate in such a manner that the several tests fuse. Following gametogenesis, the gametes derived from the several individuals are free to mingle within the brood chamber formed by the several tests. The zygotic amoebulae, which result from the union of gametes in fertilization, utilize unsuccessful gametes as food. This results in growth, so that the diameter of the initial chamber of a multinucleate microspheric individual is about equal in size to that of the mononucleate megalospheric individual.

In the sexual phase of the life cycle of *Spirillina vivipara*, a somewhat similar method of reproduction occurs. In this species, the several tests do not fuse, but remain in a horizontal position upon the substratum and become enclosed in a common cyst composed of diatom frustules and other debris held together by animal cement. It would seem that, in this and other species in which there is an association of individuals prior to gametogenesis, a chemical activator stimulates the several individuals, so that subsequent activity, including nuclear division and multiple fission, takes place at about the same time. This is further suggested by the fact that, when a microspheric individual which normally reproduces asexually, becomes enclosed within a cyst containing several megalospheric individuals about to reproduce sexually, the microspheric individual produces gametes in place of megalospheric juveniles.

In the life cycle of *Elphidium crispum*, the flagellated gametes are pelagic and there is no evidence that megalospheric individuals associate prior to gametogenesis. As a result, fewer gametes become fertilized, and the ratio of microspheric to megalospheric tests is far less than in species in which such an association takes place.

From a statistical study of populations in the sea, it was found that the life span of *E. crispum* is usually one year; and the life cycle, including a sexual and an asexual phase, requires at least two years. Under tropical conditions, it was found that the time required to complete the life cycle is reduced to one year. The life cycle of smaller species usually requires not less than six weeks to two months. Percentage frequency distribution curves for samples of populations of *E. crispum*, taken monthly for twelve months, show that growth and reproduction are limited to the colder spring months while winter dormancy sets in at a time when the water temperatures in the sea are relatively high. The food of the foraminifera, as determined from cytoplasmic inclusions and feeding experiments, consists largely of unicellular plants, and, as might be expected, those conditions in the sea, which influence phytoplankton production, indirectly influence the productivity of the foraminifera. Populations of foraminifera on the floor of the sea number from a few, to tens of thousands of individuals per square foot. *E. crispum* often exceeds 1,000 individuals per square foot, while the larger tropical species *Tinoporous* may number in excess of 100,000 individuals per square foot, on a bottom covered by a sparse growth of algae. The marked influence of slight variations in bottom sediments, food, temperature, salinity, depth, and other conditions, upon the species occurring in populations; the persistent nature of the calcareous tests in the sediments; and the changes in species that have taken place with the passing of time, make the foraminifera a useful guide to the stratigrapher, especially in petroleum geology, and to the paleoecologist, in his attempt to reconstruct the biotic conditions in geological horizon.

New methods for obtaining samples of surface sediments from the floor of the sea and the determination of populations from protoplasmic casts that remain after the sample has been treated with acid, should contribute much to our knowledge of the role of these marine rhizopods in the economy of the sea.

SECTION OF PSYCHOLOGY

MARCH 16, 1942

DOCTOR B. ROGER PHILIP, Fordham University, New York, N. Y.: *Some Factors in the Discrimination of Color Mass.* (This lecture was illustrated by lantern slides.)

Psychologists have frequently called attention to the complexity of the judgment process and have enumerated factors such as those which occur in space and time errors, contrast effects in judgment, and the series effect, to mention but a few, which exert small but significant influences on psychophysical thresholds. This paper considers the influence of certain cues based upon the knowledge of previous judgments in discriminations near the threshold. It investigates tendencies after a judgment has been made to alternate the response when the subsequent stimulus is presented; or, on the other hand, to repeat the response previously made when the subsequent stimulus is presented. In addition, it investigates the tendency of the subject to make errors in sequence when disturbed by the possibility that an error has just been committed.

The material of this experiment is similar to that which has been used in previous work of the author to study the discrimination of color mass. It consists of a series of cards, on each of which is a number of dots printed in two colors, Blue (B) and Purple (P), one of which predominates. The subject is told to judge the predominating color merely by the mass effect and without counting the number of dots. The sequences in which the stimuli were given are of importance for this study. The stimuli were given in a random order subject to the following limitations: First, in every group of 20 stimuli there was an equal number of B's and P's. Secondly, there were no more than 4 consecutive stimuli of the same type. Hence the stimuli could be alternated in the sequence BPBP, or PBPBPB, etc. or they could be repeated in doublets, triplets and quadruplets, e.g., BB, PPP or BBBB.

The primary interest of this study was to compare the tendency to alternate a previous judgment with the tendency to repeat that judgment. Thus, if the response B were given at any point in the test, an alternative response for the next stimulus would be P, and a repetitive response would be B. The repetitive type of response was first investigated. A tabulation was made for both color stimuli of the errors at each position of the doublets, triplets, and quadruplets, and of the first position immediately following these types. From a tabulation of the results, it is obvious that the percentage of error rises continuously with position throughout the doublet, triplet and quadruplet, and immediately drops when the stimulus is changed. That this increase in error is significant is shown by the fact that the critical ratios are all significant.

It must be remembered that these judgments are difficult to make, and, for the most part, are in the neighborhood of the discriminatory threshold. Even when correctly made, they are seldom accompanied by any great degree of assurance. To aid him in his judgment, the subject is eager to grasp additional cues, the influence of which is out of proportion to the precision with which they may be perceived. Furthermore, a form of the stimulus error is present. Rather than base his judgments on completely objective criteria, the subject tends to discriminate the stimuli, in part at least, by what he imagines they might be, even before they are presented. In other terms, he tends to anticipate the stimuli on the basis of some kind of probability. In the course of the experimental session, he becomes indirectly aware of the relative frequency of occurrence of doublets, triplets, quadruplets and of the alternative forms. He does not estimate this frequency in numbers, and if asked to do so, would probably be considerably in error. Nevertheless, his judgments are subtly influenced by this vague statistical knowledge. Of importance, too, is the fact that his subsequent judgments are influenced by his prior judgments. In other terms, he feels that having made a judgment on a stimulus of a certain type, the probability of occurrence of these stimuli will thereby be lessened. In reality, of course the selection of the stimulus that will be presented is, in large part, independent of previous selections, being determined only by the conditions outlined above.

As a consequence of his lack of confidence in these difficult judgments and of his tendency to use some type of probability as an aid in framing his judgments, he is prone to render a judgment counter to his discriminatory ability. His subjective attitude is antagonistic to his objective judgment. In short, while his real discriminatory ability does not change with position, his tendency to alter his judgments in repetitive types of stimuli does increase with position to the detriment of his accuracy.

In the study of the alternative types, a tabulation was also made of the errors at each position of the alternation, and also of the first position immediately following, i.e., of the first response of the doublet, triplet or quadruplet, which follows the alternation. The most interesting fact observed is that the first three positions of the alternation curve very closely resemble the form of the doublet curve. In the repetitive type of stimuli we found a clear-cut relationship between position and percentage of error — the error percentages increasing steadily with position. Hence, in the alternative type of stimuli, one might be prone to expect maxima and minima to alternate with position. Obviously this is not so. Even where an alternation does occur (in the first three positions), it is rather contrary to what one might assume from the findings of the repetitive type of stimuli. There, errors increase with position because the subject tends to *change* his judgement. The rise in error percentage at the second position and the drop in the third position in the alternation type implies that he tends to *repeat* his judgment, at least for the first three positions.

Furthermore, if, on the curve of error, percentage with position there is also plotted for comparison sake, a curve showing the number of alternations which terminate at the various positions, it can readily be noted that the general trend of both curves is similar. This fact probably implies that in the case of the alternative, as in the case of the repetitive type of stimuli, the subject becomes vaguely aware of the experimental frequency of the sequence types, and sometimes attempts to adjust his judgments to this knowledge, rather than to discriminate solely on the basis of the color mass of the stimuli. In other terms, the indirect cues that he obtains from his experience with the experimental set-up detrimentally affect his judgment, impelling him to go counter to his judgment based on discriminatory ability. This impulse is the more urgent with a consequent increase in error the longer a series continues. At times, indeed, this impulse may bring greater accuracy,—whenever his tendency to anticipate the sequence of the stimuli coincides with their actual sequence; e.g., when the stimuli are changed after a repetitive type.

What is apparent, then, is not so much that there is a conflict between an alternative tendency and a repetitive tendency in making a judgment, as an attempt to utilize a vague statistical knowledge of the sequence arrangement as a cue in framing the judgments. Cues thus obtained are often at variance with the judgments based on discriminatory ability, and progressive increases of error percentage with position arise from resultant tendencies to alternate when repetitions are required, and to repeat when alternations are required.

In the course of the experimentation, it was noticed that subjects sometimes became emotionally disturbed when making their judgments. These disturbances, chiefly noted in the harder series, seemed to cause sequences of errors. In an attempt to analyze the tendency to repeat errors, a tabulation was compiled of the frequencies of errors, one at a time, two at a time, etc., made by every subject in each series.

The X^2 test was used to determine the conformity of the experimentally found values with the calculated values of a Poisson series of best fit. The corresponding p values were also calculated. The conformity of the Poisson series is best in the easier series and there is no significant agreement in the hardest series. It may be noted that individual errors occur less frequently, and that sequences of two, three, four, etc. errors occur much more frequently than chance expectancy, as given by the Poisson series. The probable explanation for this divergence, in part at least, is that the subject becomes confused when making difficult judgments, loses confidence in his discriminatory ability especially when he has made an error, and hence tends to make two or more errors in sequence.

The results of this investigation seem to be in line with several others which have uncovered kindred effects in the influence of an unformulated knowledge of the experimental set-up as a cue in determining the response and have been interpreted on the basis of the principle of expectancy. Stated in other terms, this investigation, as well as those which are concerned with similar phenomena, is an attempt to study the adjustment of the response to the supposed probability of occurrence of the stimuli.

Distinction should be made between situations in which a discrimination can be made and where there is no guessing, and situations which evoke pure guesses, where the stimuli are presented only after the corresponding response has been made, or there is no objective means of making a correct judgment. The initial runs of mazes, and random matching experiments, are illustrations of the latter type; ordinary psychophysical experiments, of the former. Even in psychophysical discrimination, the probability factor, however, may come into play sooner or later, after the subject acquires some knowledge (not necessarily formulated) of the experimental set-up, which is used in an attempt to anticipate the stimuli. The alternative and repetitive tendencies which we have described, the expectations of the stimuli in conditioning, the patterns of response in psychophysics and parapsychology described by Goodfellow, and the ambiguous cues of Brunswik, alike fit, to a greater or less degree, in this category. In all of them, the cues for response are antecedent to the stimuli about to be presented, but are derived from, and are, hence, subsequent to other stimuli and responses.

The tendency that we have noted, in a subject to make errors in sequence when confused and disturbed, on the other hand, is due to his knowledge of responses already made, without significant reference to stimuli about to be presented. In this regard, it resembles the contrast effect described by Fernberger, Arons and

Irwin, and Preston, the series effect, and those effects which cause space and time errors. There is this difference,—the effects just enumerated are due, in part, to an absorption of the immediate stimuli into a frame of reference which alters their stimulus value. The tendency to make errors in sequence is the result of a minor emotional disturbance, whose effect on the stimulus is to increase its difficulty, by rendering less precise its corresponding frames of reference.

In the course of other experiments, where the ranking method was used with the same type of material, several factors in the discrimination process were clearly noted. They are merely summarized here, the data upon which the conclusions are based are given elsewhere.

1. In the discrimination process, there is quickly established a standard, a frame of reference from which judgments are based, which may be (a) general and remote, due to past experience; or (b) particular and proximate, originating within the experimental series.

2. This standard is subject to disintegration and interaction. Hence originate the time errors which reflect the distortion of the judgments from the standard as indicated by the skewness of their frequency curves.

3. Serial effects, whose strength is a function of their deviation from the frames of reference, depend upon inhibitory influences of neighboring stimuli, and, in turn, cause variation in the accuracy of the serial judgments.

4. Generalization effects, which have an exponential gradient, indicate the spread of erroneous choices to surrounding stimuli. The similarity of the stimuli and the difficulty of the task make for inhibition with a consequent diminution in the discrimination and an increase in the generalization.

5. Variations in the attitude or set, which is a powerful factor in selective discrimination, cause variations in the frame of reference and distortions in the judgment.

6. The principle of expectancy may be used to explain anticipatory and repetitive tendencies, based upon a vague, sub-

liminal awareness of the relative frequencies of the various stimuli.

7. Emotional upsets, when the subject becomes uncertain of his judgments and has the feeling of having made an error, diminish the discriminatory ability and are the basic cause of sequences of error beyond normal chance expectancy.

SECTION OF ANTHROPOLOGY

MARCH 23, 1942

DOCTOR CONRAD M. ARENSBERG, Brooklyn College, New York, N. Y.: *Irish Rural Social Organization.*

The problem presented by the statistics of population in the Irish Free State offers an excellent example of the way in which the techniques of field-study and interpretation of custom, developed first among primitive peoples in social or "cultural" anthropology, can be used profitably to gain an understanding of economic and social phenomena in the more complex societies of contemporary civilization.

Irish demography presents a picture of conditions unique in the civilized world. The population has declined from eight million in the year of the first census, 1841, before the Great Famine, to a mere three million in 1941. Along with this decline, has gone a steady rise in the average age of first marriage, until now, in Ireland, men and women marry at a later date than anywhere else. In addition, the proportion of those who escape marriage altogether has risen progressively, until today more of the men and women of all ages remain celibate in Ireland than in any other country for which we have adequate records.

Such a situation presents an intriguing problem in the interpretation of human custom, and in the adaptation to customs of human beings. It is not a biological or nutritional matter. Once marriages do take place in Ireland, they are as fecund as among other human races. Furthermore, there seems to be no likelihood of a decline of population among the people of Irish descent transplanted overseas. In fact, this demographical picture has included another characteristic facet until the outbreak of World War II. Though emigration did not sustain the immense volume it showed during and just after the Famine years, it has maintained a steady flow for over a hundred and fifty years, playing its part all through the rise of the conditions just described.

The usual explanation, upon which many schemes of remedial action and governmental intervention, both British and native, have been based, is that such demographic conditions must be the result of chronic, endemic poverty. The first hypothesis that springs to mind is that human beings forego matrimony and parenthood so long, and in such numbers, only when they must.

But the records soon demonstrate that such demographic conditions do not spring directly from economic causes. In the first place, the conditions are characteristic of the small farmers, peasants, who, while not the most prosperous of Irishmen, are not the poorest class in the land. The landless laborers, particularly of the towns, chronically unemployed and on relief, show no such delay in marriage and widespread celibacy.

Furthermore, the characteristic conditions have been steadily on the increase during the same hundred years that has brought the level of living, savings, and economic success among the small farmers of Ireland from the nadir of the Famine to its highest recorded state. The conditions are earmarks, not of poverty, but of progressive prosperity.

Another easy explanation used to run, before the establishment of the Free State and self-government, that these conditions arose out of a flight from intolerable conditions at home. Yet the same trends persist through the whole period and show no change whatsoever corresponding to the shift from foreign to native rule. Indeed, both governments persisted in treating the problem as an economic one and adopted many palliative measures. The remedies proved equally useless, regardless of their origin at home or abroad.

Nor could the explanation be any lack of opportunity for life careers. For the period of the growth of these demographic conditions was also the period of the emancipations of the Catholics, the throwing open of professional, mercantile and civil service posts to the sons and daughters of the local farming population, and the filling up of these walks of life—till 1870 reserved for the sons of the Protestant and "English" Ascendancy (i.e., the descendants of the Cromwellian colonists became landlord aristocrats), with Catholic "Irishry." The demographic figures correlate not with restricted opportunity, but with an open class system and the existence of *carrières ouvertes aux talents.*

In the course of the Harvard Expeditions in Ireland, in the years 1932–1935, it was felt that this problem of population warranted special attention from the members of the expeditions conducting social-anthropological research into present social life, as distinct from those conducting archeological and somatological research. But it was not until a thorough exploration of local custom and social organization was completed among the small farming population in the areas of the country where relatively more of the ancient tradition survives, that a satisfactory interpretation began to appear. The possibility of correlating statistical indices for the whole country soon became clear from the results of participant observation, interviewing and case study, and community structure analysis in the field.

Irish rural life is based upon the individual-family farm (Meissner's *Einzelhof*), an ancient "Celtic" or West-European form of land-settlement. Today, as in many parts of the United States, these single-family farms are self-sufficient economic units. Practising a mixed, in this case, primarily pastoral economy, the united efforts of a man, his wife, perhaps a sibling of either, and their children of both sexes and all ages, support a single household from the produce of milk cattle and a potato garden. The household enters the stream of commerce only to dispose of its annual surplus of calves (which become the beef cattle Ireland supplies the English table), milk, and eggs. Even where, as often today, clothing, wheat-flour, tea and sugar are bought in the market towns, the outline of family self-subsistence remains little changed.

Such a basis of subsistence economy in individual family households, each one very much a duplicate of the other, puts a very definite imprint upon community life. The principal differences in status (within the peasant class) depend very largely on age-grading, in which the members of an older generation are farm family heads, men and women "of the house." All others, the young, the landless, and the few followers of rural trades, supplying the farm families locally, but without farms of their own, are grouped together in the community as "boys" and "girls," inferior in status. Tradition, folklore, and supernatural belief, as, for example, an identification between the misnamed "fairies" and the dead ("old men and women"), support this stratification of the community upon the basis of age. Fundamentally, of course, it is to be traced directly into household and family economy, where the "old man" and "old woman" direct and control the united efforts of the self-subsistent group.

Ties between the self-subsistent family units, other than the links of common position among the family heads, namely, the "old people," depend, then, upon the solidarities originally established among brothers and sisters, as boys and girls in the united family enterprise. In the transition from generation to generation, these boys and girls are dispersed from the original household through emigration, movement into town occupations, or out-marriage. What cooperation exists in agriculture, and nearly all the ties of mutual aid and obligation between families, thus follow these ties. "Friendship," as the bonds between cousins of first, second, and third degree are called, remains the most complete, and, in many instances, the only basis of any contact and solidarity between families. As such "friendship" depends upon common descent, its existence supports the misimpression that the "clan" continues to exist in Ireland. Actually, the patronymic line is only one, though symbolically the most important one, of the lines of common descent, which unite the "friends" or cousins of the same generation of the bilateral kinship group. The long-continued occupance of the land by a single family, and the inheritance of family holdings by single heirs (nowadays, the usual practise), have identified blood relationship and membership in social groups, occupations, and a community, to a point where kinship ties outweigh any others.

As a result, an exaggeration of the European stem-family or *famille-souche*, first described by LePlay, has appeared. The setting up of the new household of the heir comes to involve a marriage, which introduces a bride to become the new "woman of the house," and which disperses the remaining children who do not inherit. Such a marriage, known locally as "making a match," exchanges a farm inherited by the groom for a "fortune" or dowry supplied by the bride. In the exchange, a complete transformation of the relationships and statuses of all the household members takes place and is provided for. The former family heads, father and mother of the heir, retire from active command, move into the status of "old people," and divide up all the surplus accumulated by the united efforts of the family (beyond the farm and its capital, which go to the heir) into "fortunes" providing dowries for daughters and starts-in-life, such as fees for professional training, apprenticeship, or emigration, for sons. Since such provision occurs only at the match, or in immediate anticipation of it, the marriage of an heir comes to be the most crucial point of family transition, and one in which all the life-interests of the corporate family group are served. Again, since local prestige depends upon one's family's "land," and since the size of the provision made for children is related and equated to the return of labor from the "land," the success with which such a transition is effected also becomes a chief source of prestige. Since emigrants into the towns or overseas occupations (education for which is so painfully paid for in the efforts of the whole family at home) are all dispersed brothers and sisters bound to the parents and the heir at home by the ties of "friendship" or kinship, which alone allow mutual aid, solidarity, and common prestige, a successful dispersion of the family members, in anticipation of transmitting a farm to an heir and his bride, brings direct and prolonged advantages to the kinship group at large, and to the individual-family reestablished on the home farm.

Around such a nucleus of family transition, reformation, dispersal, and creation of extended kinship obligations, all involved in match-making marriage, a great deal of local custom and community life revolves. Marriage becomes the central focus of both individual and group sentiment and aspiration, and it becomes very much indeed a transition in corporate existence in rural Ireland. That marriages should be so long delayed, and so rare in a rural social organization where only an heir can marry, and then only when a family is successfully and advantageously dispersed, turns out, then, no longer to be surprising.

In the light of such knowledge, indeed, it can be seen, at once, why celibacy and delayed marriage in the rural population of Ireland have become a concomitant of prosperity, not poverty. The demographic conditions represent the reaction of an ancient familial system of social organization to the opening up of bourgeois, middle-class occupations, requiring the considerable preparation and education which could only be brought by the surpluses accumulated by an entire working household, to the sons and daughters of small farmers trained in the ancient tradition. New economic conditions and new opportunities in middle-class life did not transform the old Irish family system. The family system adapted itself to the new conditions and the new opportunities. Late marriage and a high proportion of celibates are the indices of that adaptation. They mark the scramble for better jobs, for higher positions in life, and for superior opportunities for dispersed children, to which the new conditions lead. The anthropological evidence is that changing economies do not transform custom. On the contrary, custom persists and forces economic change to conform to its mold.

SECTION OF OCEANOGRAPHY AND METEOROLOGY

MARCH 6 AND 7, 1942

Conference on "*Boundary Layer Problems in the Atmosphere and Ocean.*"

The new Section of Oceanography and Meteorology started with a Conference on "Boundary Layer Problems in the Atmosphere and Ocean," the first of a projected series. Doctor C. G. Rossby, Institute of Meteorology, University of Chicago, Chicago, Illinois, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Address," by C. G. Rossby.

"On the Ratio Between Heat Conducted from the Sea Surface and Heat Used for Evaporation," by H. U. Sverdrup, Director, The Scripps Institution of Oceanography.

"Evaporation from Land Surfaces," by B. Holzman, U. S. Weather Bureau.

"Turbulence and the Transport of Sand and Silt by Wind," by Anton A. Kalinske, Iowa Institute of Hydraulic Research, University of Iowa.

'Sources of Atmospheric Heat and Moisture over the North Pacific and North Atlantic Oceans," by Woodrow C. Jacobs, U. S. Weather Bureau.

"The Effect of a Gradual Wind Change on the Stability of Waves," by B. Haurwitz, Massachusetts Institute of Technology.

"Boundary Layer Problems Involved in Snow-Melt," by Phillip Light, U. S. Weather Bureau.

"Generalization for Cylinders of Prandtl's Linear Assumption for Mixing Length," by R. B. Montgomery, New York University.

SECTION OF PHYSICS AND CHEMISTRY

MARCH 27 AND 28, 1942

Conference on "*Color, Constitution and Reactions of Dyes.*"

The Section of Physics and Chemistry held a Conference on "Color, Constitution and Reactions of Dyes," as the fourth in the series for the Academic Year of 1941–1942. Professor Victor K. LaMer, Columbia University, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introduction," by Victor K. LaMer.

"Absorption and Resonance in Dyes," by L. G. S. Brooker and collaborators, Eastman Kodak Company.

"Theoretical Discussion of the Absorption of Polymethine Dyes," by A. L. Sklar, Catholic University.

"Ultraviolet Absorption of Organic Molecules," by W. H. Rodebush, University of Illinois.

"Influence of Position of Substitution on the Absorption Spectra of Isomeric Dyes," by Wallace R. Brode, Ohio State University.

"Photooxidation, Photoreduction and Photogalvanic Effect of Dyes," by E. Rabinowitch, Massachusetts Institute of Technology.

"Kinetics of Carbinol Formation in Triphenyl Methane Dyes," by Seymore Hochberg, Marion Barnes and Victor L. LaMer, Columbia University.

"Regeneration of Alkali Faded Brom Phenol Blue in HCl Solutions," by E. S. Amis, Louisiana State University.

NEW MEMBERS

Elected March 26, 1942

ACTIVE MEMBERSHIP

Buehler, Charlotte Bertha, Ph.D., Visiting Professor, Clark University, Worcester, Mass.; Lecturer, College of the City of New York, New York, N. Y.

Clark, Frances H., Sc.D., Metallurgist, Western Union Telegraph Company, New York, N. Y.

Claude, Albert, M. D., Associate, Rockefeller Institute for Medical Research, New York, N. Y.

Cox, Edward H., D.Sc., Professor, Chemistry, Swarthmore College, Swarthmore, Pa.

Crane, E. J., D.Sc., Editor *Chemical Abstracts*, Ohio State University, Columbus, Ohio.

Darken, Lawrence Stamper, Ph.D., Physical Chemist, U. S. Steel Corporation, Research Laboratory, Kearny, N. J.

Dingle, John Holmes, Ph.C., Sc.D., M.D., Instructor and Francis Weld Peabody Fellow in Medicine, Instructor in Bacteriology and Immunology, Harvard Medical School; Assistant Physician, Thorndike Memorial Laboratory; Assistant in Medicine, Boston City Hospital, Boston, Mass.

Dorcas, Merrill J., Ph.D., Technical Consultant, Manager Specialties Department, Carbon Sales Division, National Carbon Company, Inc., Cleveland, Ohio.

Harvey, Burwell T., A.M., Professor, Chemistry, Morehouse College, Atlanta, Ga.

Holmes, Ralph J., B.S., Lecturer, Mineralogy, Columbia University, New York, N. Y.

Kertesz, Francois, Sc.D., Ch.E., Research Chemist, E. I. du Pont de Nemours and Co., New York, N. Y.

Kurrelmeyer, Bernhard, Ph.D., Professor, Physics, Brooklyn College, Brooklyn, N. Y.

Lorentz, John J., M.S., Instructor, Biology, St. John's University, Brooklyn, N. Y.

Renn, Louis Hancock, Public Relations Counsellor, New York, N. Y.

Russell, John B., M.S., Assistant Professor, Electrical Engineering, Columbia University, New York, N. Y.

ASSOCIATE MEMBERSHIP

Coraje, Eliseo Jesus, D.Sc., LL.D., Anthropology, Psychology, Retired, San Diego, Calif.

Corwin, Alsoph H., Ph.D., Associate Professor, Chemistry, Johns Hopkins University, Baltimore, Md.

Coryell, Charles D., Ph.D., Assistant Professor, Chemistry, University of California, Los Angeles, Calif.

Hamre, Christopher J., Ph.D., Professor, Zoology; Chairman of Department, University of Hawaii, Honolulu, T. H.

Lowenhaupt, Henry Stewart, B.S., Graduate Student, Yale University, Sterling Chemistry Laboratory, New Haven, Conn.

Newman, LeRoy Lafayette, Jr., M.S., Junior Physicist, Southern Regional Research Laboratory, Schenectady, N. Y.

Smoluchowski, Roman, Ph.D., Research Physicist, General Electric Research Laboratory, Schenectady, N. Y.

Spurlin, H. M., Ph.D., Head, General Research Group, Hercules Experiment Station, Wilmington, Del.

West, Cutler D., Ph.D., Research Chemist, Polaroid Corporation, Cambridge, Mass.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 4 MAY, 1942 No. 7

SECTION OF BIOLOGY*

APRIL 13, 1942

DOCTOR KEITH R. PORTER, Rockefeller Institute for Medical Research: *Developmental Variations Resulting from Various Associations of Frog Cytoplasms and Nuclei.* (This lecture was illustrated by lantern slides.)

The nuclei and cytoplasms of dividing or reproducing cells are commonly recognized as being mutually dependent, but the characteristics of the relationship are not known. During the early cleavage of the frog's egg, there is a pronounced increase in the number of nuclei and the total quantity of nuclear material. The materials for this duplication of nuclei are apparently derived from the cytoplasm, but the processes involved, what, in fact, the nucleus contributes to or receives from the cytoplasm, what characteristics of the one determine how the other shall react, are examples of questions for the answering of which there is little available information. It was to examine materials and methods

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*The Section of Geology and Mineralogy met April sixth. No abstract was received from Professor Kirk Bryan, who gave an address entitled, "The Geologic Antiquity of Man in North America," illustrated by lantern slides.

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which might provide at least a possible approach to the investigation of problems such as these that the experiments to be described were undertaken.

The method employed is that often used in examining the relationship between two coexisting entities—one is varied while the other is kept constant. Following insemination of the frog's egg, the egg nucleus can be successfully removed and the egg cytoplasm is left with only the sperm nucleus present and functional. If sperm from a variety of species or races are used for insemination, it is obvious that a single type of cytoplasm can be combined with a variety of nuclei. By the same procedure, the cytoplasms can be varied and the nucleus kept constant.[1] The resulting embryos develop with only one-half the normal number of chromosomes, and are referred to as androgenetic heterospermic haploids. The developmental picture, the early embryology of these haploids, becomes for these experiments the index in terms of which the nuclear or cytoplasmic differences are described.

When the nucleus and cytoplasm which are brought together to form a heterospermic haploid are derived from distinctly different species, development generally ceases at a very early stage. This has been demonstrated in the investigations of Boveri, G. Hertwig, P. Hertwig, Baltzer, Horstadius, and several others. It was therefore of some interest to find that germ cell nuclei and cytoplasms derived from two distinct forms of frog occurring separately in northern Vermont and southeastern Pennsylvania, when combined reciprocally, yielded heterospermic haploid embryos which developed almost as well as the homospermic or control haploids, i.e., into swimming tadpoles. Such departures from the normal as were shown by these heterospermic haploids indicated the existence of differences (probably slight) between the germ cell nuclei and cytoplasms of the two forms. It was suggested that the two forms used were not distinct species, but races of the same species, best called *Rana pipiens*, and that the nuclear and cytoplasmic dissimilarities which found expression in the early development of the heterospermic haploids might be related to differences of an adaptational character which permitted the egg of the Pennsylvania form to develop over a different (higher) temperature range. This latter possibility suggested that between the Pennsylvania form and still more southern dwelling forms of this same type of frog, differences similar to those found between Pennsylvania and Vermont forms might be demonstrable. It has been shown that a Pennsylvania nucleus in Vermont egg cytoplasm, *(vt.)pa.*, produced an embryo with abnormally large anterior structures. Would the combination of a nucleus from a Florida form and Pennsylvania cytoplasm, *(pa.)fla.* show a similar departure from the normal?

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[1] Control of nuclear or cytoplasmic variation is at present limited to a selection of sources from which the nuclei or cytoplasms are derived. Since methods are not available by which controlled changes can be made in nuclei and cytoplasms, it is necessary to seek naturally occurring variations and to attempt to determine their character.

The sources of the more southern forms were largely determined by the locations of frog dealers. Hence southern Louisiana and southern Florida provided two additional forms. Nuclei and cytoplasms from the four sources,—Vermont, Pennsylvania, Louisiana, and Florida, were combined in all possible ways, and the development of the heterospermic and homospermic control haploids was carefully followed and compared.

The forms of leopard frog used are quite similar and replace each other geographically. Laboratory tests of hybridizability have shown that adult diploid hybrids develop from all combinations, and that these hybrids are fertile. For these reasons and others, the four forms are considered as representing geographic races of the same species.

The results of combining the various sperm nuclei and egg cytoplasms can be summarized by a few generalizations:

1. If the nucleus used is derived from a more southern race than that from which the cytoplasm is obtained, the heterospermic haploid embryo develops with abnormally large anterior structures. This answers in the affirmative the query made above, i.e., the *(pa.)fla.* embryo shows abnormalities similar to those shown by *(vt.)pa.* If, on the other hand, the reciprocal combination is made, if the nucleus is derived from a more northern

race than the cytoplasm, the anterior embryonic structures are abnormally small. Thus, reciprocal combinations of nuclei and cytoplams from the same two races depart from the normal in apparently opposite directions.

2. It is roughly true that the more widely separated in a north-south direction the sources from which the combined nucleus and cytoplasm are derived, or perhaps more exactly, the greater the climatic difference between the sources, the greater the departure from the normal shown by the resulting embryo.

3. If a series of four embryos, all having the same type of nucleus but different cytoplasms, are compared relative to the size or form of the same embryological structure, it is observed that they can be arranged in a gradient of size or form which roughly parallels the north to south or climatic distribution gradient of the sources. This similarity in type of abnormality, with differences chiefly in degree, seems to suggest that the cytoplasms differ not so much in actual components as in, perhaps, the physical characteristics of the same components. Furthermore, in whatever respect the cytoplasms differ, the demonstrated differences would appear to be related to the north to south geophraphic distribution of the sources.

4. The same statements made above, under 3, relative to cytoplasmic differences can be made relative to nuclear differences.

5. The control embryos are observed to be almost identical in their development. Up to now, comparison of them has yielded no common differences which can be related to differences shown by the hybrids.

6. Cytological examination of the hybrids reveals, in addition to embryological dissimilarities, clear differences in mitotic activity. For example, a Florida race nucleus in Vermont cytoplasm divides much more rapidly during early development than does the Vermont nucleus in Florida cytoplasm.

Certain nuclear and cytoplasmic factors or components involved in early development and in determining rate of nuclear division, having been shown to differ in the germ cells of these four races, it becomes of interest and importance to consider the possible nature of the differences. Among such differences as are known to exist between these germ cells, those which represent adaptation to the different environmental temperature conditions seem most significant. The eggs, for example, are prepared to develop at quite different temperatures, low for the northern race and progressively higher for the more southern races. If this fact is considered in conjunction with the obvious parallelism which, as mentioned above, apparently exists between the demonstrated gradient of nuclear and cytoplasmic differences and the gradient of north to south or climatic distribution of the races, it is difficult to avoid the assumption that the factors involved may differ in their temperature characteristics. It is possible that further examination of the material will confirm or deny this assumption and reveal whether or not the material is capable of yielding any precise information on the relative roles of nucleus and cytoplasm in dividing cells.

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SECTION OF PSYCHOLOGY

APRIL 20, 1942

DOCTOR LIVINGSTON WELCH, Institute for Research in Child Psychology, Hunter College: *Some Aspects of the Development of Inductive Reasoning.* (This lecture was illustrated by lantern slides and motion picture film.)

INTRODUCTION AND METHOD

A formal definition of inductive reasoning can be presented in the short proposition, "A process of arriving at generalization from particular cases." An investigation, however, of what circumstances and abilities contribute to success in a reasoning situation, covers almost the entire field of psychology. Memory, emotional stability, interest, persistence are some of the major factors which have a direct bearing on this problem. All of these make it possible for the reasoner, among other things, to perceive certain essential similarities—to group together or to generalize certain essential characteristics and relationships. This brings up the question as to what kinds of generalization the reasoner may be required to make in dealing with the principle of inductive reasoning. Such a list would be almost endless and by itself might tell us very little. Any answer to this question, in whole or in part, requires a departure from speculative to experimental inquiry.

During the past three years, Dr. Louis Long of City College and I have been conducting a series of experiments concerned with a limited number of these factors which influence generalizing in the field of inductive reasoning, between the ages of 3 to 13 years. In all instances, the procedure began with a matrix problem which made use of concrete material and required the application of Mill's Joint Method of Agreement and Difference. The relationship under investigation was between cause and effect. On the board of the apparatus there were three rows. Two blocks with plugs were inserted simultaneously into the holes of each row. When the experimenter had finished his demonstration the bulbs in two of the three rows were lighted. The causal blocks responsible for the light were the blocks with the same picture of an animal found in the two lighted rows and absent from the row that was not lighted. This reasoning situation is comparable to one of the many classic problems which required the use of Mill's Method of Agreement and Difference—the discovery of the cause of yellow fever. The one factor preceding all cases of yellow fever and absent in all instances where the fever did not develop was the bite of a certain type of mosquito.

During the maximum number of 30 trials of the matrix problem, account was taken of the types of hypotheses the subjects employed in their attempts to arrive at a solution and of their success in verbalizing the principle after they had discovered it.

Successful subjects were given the opportunity to generalize the same principle, when the original blocks with pictures of different species of animals were substituted by those of different colors. Later, the children were called upon to generalize the principle to a similar situation where the phenomenon of color was substituted by that of direction.

In another series, the children were given material of varying degrees of abstractness. An illustration of a causal factor at the original object level would be blocks which had the same photograph of a horse in the two rows with lights. At the first level of abstractness, the causal blocks might be those which had pictures of different animals, members of the class of animals, while, at the second level of abstractness, the causal blocks might be a horse and a woman, members of the class of living beings. At this last level, the classes involved were the class of living beings (animals and people), the class of food (vegetables and fruits), and the class of vehicles (boats and land vehicles).

All of the problems so far described involve the use of three dimensional material. Our next investigation concerned the children's ability to generalize the principle from three dimensional material to pictorial material, and, lastly, to language material. The pictorial material consisted of photographs of six different kinds of vegetables in place of six different species of animals on separate blocks. In each trial, three of these were presented in pairs of two, three rows deep. Opposite each of the three rows were pictures of a girl. Two of these presented the girl sick in bed. The third showed her jumping a rope. The general set-up was the same as that of the block form and block color problems.

The linguistic material was presented in the form of a paper and pencil test. This series made use of different types of foods, while the effect was illness in two out of three instances. Each problem was printed in three rows—two kinds of food which were followed by illness in two instances and not in the third instance. The antecedents in this series were increased from two to three, and then to four. These linguistic problems also include those involving two levels of abstract concepts. For example, if originally oysters were responsible for illness, at the first level of abstractness the cause might be two species of shell fish, while at the second level of abstractness it might be any species of fish. The last type of problems in this series involved both an increase in the number of antecedents and in abstractness.

Results

1. *Some Causes of Reasoning Failure:*

(a) It often happened that the child focused his attention on the wrong section of the apparatus. In some instances 15 to 20 trials were given before he shifted his attention to that part of the board where the solution of the problem might be found.

(b) Another factor related to the classifications used in the hierarchy tests. Decrease in the scores of the 2nd hierarchy were sometimes caused by cross classifications of the 1st hierarchy concepts. For example, a bus might be picked as the causal factor because a man rides in an automobile, the lights being on in the rows containing these 2 blocks. The correct solution would be a row boat, because the automobile and the battleship are in rows with lights, whereas the selection of the choice block of the boy would be incorrect, because a horse is in the row without a light.

2. *Relationship between I. Q. and Reasoning.*

The correlation in all instances between I. Q. and reasoning was very low. The correlation between M. A. and reasoning, when C. A. was removed, and also the correlation between C. A. and reasoning, when M. A. was removed, were likewise low. Still the general results indicate that a child has to have a certain amount of C. A., and all that this implies in terms of experience, before he is capable of solving the type of problem used in this test.

3. Discovery of the principle in its original form, in the majority of cases, was not possible before 9 or 10 years of age. Children at this age and older were more resourceful in thinking up new hypotheses and were quicker to abandon an unsuccessful hypothesis than the children from 3 to 6 years of age.

4. Success in generalizing the principle when the characteristic of form was substituted by other phenomena and media made up the sequence color, pictures, words, direction—in this descending order of success.

5. Generalizing the principle, however, from three dimensional material to pictorial and language material was not as difficult for the subjects as generalizing to material that involved abstract concepts. Success in connection with this factor increased with age. In a marked degree this was first prominent from 10 to 11 years of age.

6. An increase in abstractness caused greater difficulty than an increase in the number of antecedents, while still greater difficulty resulted from a simultaneous increase of both these factors.

7. Lastly, while many children solve these problems without being able to give an adequate explanation of the principle, superior performance in generalization was given by those who verbalized adequately.

SECTION OF ANTHROPOLOGY

April 27, 1942

Doctor Eliot D. Chapple, Harvard University: *The Measurement of Interpersonal Behaviour.* (This lecture was illustrated by slides.)

The measurement of human relations arose, some five years ago, as a result of certain difficulties which Dr. Arensberg, who had a hand in developing these methods, and I experienced in trying to analyze our field observations and interviews.

As anthropologists, we were concerned with trying to understand what people did by watching their behaviour and listening to what they said. To help our understanding, we used terms like solidarity, integration, superordination and subordination, social mobility, anomie, Oedipus complex and repression. Ten years ago, we firmly believed that these terms were perfectly clear and that their meanings were equally clear to others.

After several years of work on our material, we gradually came to realize that all these terms were, for us at least, subjective in character. When we came upon a case we had labelled solidarity in the family, for example, and another which we regarded as an example of solidarity in the church, we found it rather hard to know what they had in common. Moreover, when we asked ourselves how we recognized solidarity even in the single instance, we found that we were depending upon our interpretation of particular words or actions which, in another case, we might not treat in the same way.

So we began to ask ourselves, what are the stigmata by which we recognize solidarity or superordination in the relations of people? It was clear that each of these qualities varied in amount under different conditions. Could we get some kind of crude measuring rod by which the amount of solidarity, for example, could be estimated and compared from relationship to relationship? Going back over our best material, that is, over those cases which we intuitively felt made the most sense, we found that, in a rough approximation, the length of time that two people were together seemed to determine the degree of their solidarity. But this length of time could itself be broken up in several ways, in terms of the length of each event between the two of them, the frequency with which these events occurred, and so on.

We then began to test this idea out on our material and on cases from the literature, and we saw that it afforded us an objective method of determining the "strength" of relationships. Moreover, we also began to understand the significance of the techniques and activities which make up what the anthropologist calls the culture of a people. We saw that the performance of these routines determined which people should come together and for how long, and provided the framework for the habitual system of relations making up the social organization of a people. The kinds of situations which anthropologists describe under such headings as patrilocal residence, joking relationships, mother-in-law tabus, and so forth, could be given a quantitative value in terms of the number, frequency and duration of events between specific people.

When we turned to other terms we had found of great subjective value, such as superordination and subordination, we saw that these, too, could be defined objectively. The superior person was he who started action in an event, and the person having the highest degree of superiority was he who initiated action for all the members of his group with a high frequency. We found it convenient to differentiate between events involving only two persons from those in which more than two were involved, since the latter type were those which defined lines of authority. In this case, as in the previous one, we found that the cultural techniques determined, in part, who was to initiate action and to what people; whether it was in the routines of preparing meals, working in an assembly line, or conducting meetings. Ceremonies and rituals, from this point of view, were not merely of interest from the symbolic aspect, they had greater significance in that they provided specific persons an opportunity to get responses out of others.

Finally, in the course of this crude beginning, we began to see that individuals themselves differed in their capacity to initiate action, in the amount of time they spent with others, etc. In other words, there were marked differences between individuals in this aspect of what is called their personalities.

Once we could apply these kinds of measurements to the relations of persons, once we had defined for ourselves these operations of frequency and duration of events, initiations of action, and the like, we discovered that our own material was only occasionally adequate for this kind of analysis. We then began to make new observations in factories and other small groups in order to test these methods for ourselves. It very soon occurred to us, after some field experience, that it would be a great help to an observer if he could eliminate notebook, pencil and stopwatch and make his observations mechanically. Accordingly, we devised the first of several instruments with which we have been able to measure human relations.

In passing, however, it may be worth mentioning that once we began to define our measurements, the old terminology began to drop out. This was not done deliberately. We found, not merely that it was more convenient to use terms which referred to the operations used, but also that the old terms could only be applied after an inordinate amount of time had been spent in explaining how we defined them Since the result of such an explanation usually was that the explainee complained bitterly, "That's not what I mean by superordination," we found it easier on everyone to give up the attempt. The chief loss has been that we have not been able to provide any bridge from the old terminology to the new for those who like to move by easy stages. In any case, the important thing is the development of operations by which the relations of individuals and groups of individuals can be measured, for from them, an operational account of society is possible.

Once you turn from a rough description of the length of time two people are together and try to record what goes on during the event between them, you are compelled by the nature of the material to record their interaction. By interaction, I mean the alternation of action and inaction by an individual, ordinarily manifested in the presence of other individuals, in such a way that, in a rough approximation, the action of one individual occurs during the period the other is not acting. Both actions and inactions may be as short as a fraction of a second. In the ordinary non-pathological individual, they may last several minutes. The actions may be vocal or non-vocal or both. In the human, they consist essentially of speech and gestures. The measurement of the interaction of two or more persons involves recording the length of each action and inaction for each person, and then measuring the degree to which these action-inaction sequences are synchronized.

The recording machine consists essentially of a moving tape driven at a uniform rate of speed on which lines are drawn when the observer presses a key. When a person starts to act, the observer presses down the key assigned to that person and holds it down until he stops acting, when the key is released. Each other person is treated in the same way. From a tape for two persons, we are able to measure off not only the length of each action and inaction, but also the length of each interruption as well as the length of time that one fails to respond to the other. We also know which person was responsible for the interruption or failure to respond, and which of the two outacted the other after an interruption, and which, after a period when both were inactive, initiated the action.

In the series of measurements reported on, consisting of normal individuals and patients from the psychiatric ward and out-patient clinic of the Massachusetts General Hospital, we were primarily concerned with the interaction of members of pairs, and only incidentally mentioned our work with more than two individuals. We concentrated on pair-relations because they are the most frequently occurring type, and because we wanted to discover the differences that individuals exhibited in their capacity for interaction. By so doing, we would be able to define the properties of the principal building-block of organizations,—the relation between two persons. Once this had been defined, the more complex case involving three or more individuals could then be dealt with.

Each set of measurements has been described by the use of an alternating series. An alternating series is made up of a sequence of alternately positive and negative terms which is summed cumulatively. If we let the actions be positive and the silences or inactions negative, then the relative length of action to silence determines the slope of the curve (given an arbitrary value for each term of the series on the abcissa).

The use of the alternating series is based upon the assumption that the amount of activity and inactivity of an individual is constant within limits. If there is an increase at one period during the day, there will be a decrease at another, and this assumption is based upon physiological evidence to be referred to later. Since the interactional situation requires an alternation between action by one individual and action by the other, some kind of balance has to be obtained in the durations of activity manifested by each individual. The alternating series provides a continuous measure of the changes in this output from individual to individual and of the adjustments which each has to make to the other.

Four curves are constructed for each individual from each observation, all consisting of alternating series. The first is the activity curve,—the individual's actions minus his silences. The second consists of the individual's interruptions of the other person minus his failures to respond to the other person,—his adjustment curve. The third consists of the other person's interruptions minus his failures to respond. The fourth, called the initiative curve, consists of a numerical count of the individual's continuations of action after an interruption (when the other person becomes silent) plus his initiations of action after both are silent *minus* the other person's continuations of action together with his initiations of action.

In the series of observations reported upon, marked differences were found both between normal and abnormal individuals as well as between normals themselves and the abnormals themselves. Dealing with the abnormal patients first, the manic individuals were characterized by very steep positive activity slopes, while the psychotic depressions had equally steep negative slopes, on the average over 85 degrees. In the manic, the adjustment curve of the patient was positive, indicating consistent interruption of the doctor. In the depression, the 'adjustment curve was negative, showing pronounced latency of response.

Schizophrenic patients showed moderate activity curves, with many instances of marked drops to negative values, while the patient's adjustment showed marked negative adjustment curves, showing the characteristic latency of response. The difference between the depression and the schizophrenic lies in the relationship of the activity and adjustment curves. In the former, the activity curve lies at a greater negative angle than the adjustment curve. In the latter, the position is reversed, and the activity curve is very frequently positive in value. The final instance of psychosis illustrated was that of the paranoid state, in which alternations between high activity and marked inactivity seemed to be characteristic without any pronounced latency of response.

Four types of psychoneurosis were illustrated,—hysteria, anxiety, obsession and depression. In these instances, the activity curve moved progressively from positive to negative values; the hysterias showing marked activity, between 80 and 84 degrees of slope; the anxieties varying around 45 degrees positive; the obsessions showing similar negative values, and the reactive depressions negative values similar to those in hysteria.

Characteristic of all the psychoses and psychoneuroses was the lack of adjustment as measure by the occurrence of interruptions and failures to respond, with the normal individual exhibiting a high frequency of periods in which neither interruption or failure to respond occurred. Also characteristic of the normal individual was a greater variability in the activity slopes. The abnormal individuals present day after day approximately the same values. The normal individuals vary widely in their activity rates, although that variation occurs within well-defined limits. Moreover, different individuals suffering from a given ailment show little variation between themselves, unlike the normals.

In a series of over 250 normals, we have found that the extreme activity slopes of the manics, hysterics, psychotic and reactive depressions do not occur. In all cases, patients so diagnosed showed far steeper slopes negatively or positively than those found in normal individuals. All other types, paranoias, schizophrenias, anxieties, obsessions, fall within normal limits on the activity curves. Diagnostic criteria in these latter cases are dependent upon the shape of the activity curve and, in particular, upon the interrelations of the other curves with each other and the activity curve.

Several instances of psychosomatic disorders were also illustrated to show the variations in interaction rates obtained under such conditions. The push of activity in a patient with a history of migraine, having a neuro-angiotic edema, and the depression-like curve obtained in ulcerative colitis were shown, together with an instance of an individual recovering from the latter ailment who had an activity curve comparable to that found in anxieties.

Experimental modifications of these curves were shown. First, through changes in the interaction rates of the interviewer and, second, by the use of drugs affecting the autonomic system. In the latter case, mecholyl produced a marked increase in the activity rate of a normally active person, while adrenalin changed the activity to a definitely negative slope on the order of that produced in the depressions.

Finally, differences in individuals having the same activity rate were shown based upon the length of the units of interaction (made up of an action and subsequent inaction), so that we obtained a measure of the relative speed with which a given amount of activity was produced; and also differences in the capacity of individuals to obtain responses from groups of individuals, which provide one with an effective measure of leadership in terms of interaction.

DISCUSSION

The study of interaction is only a part of the wider study of the activity of organisms, which may be divided into two major subjects: first, the study of spontaneous activity, that is, activity manifested by the organism under constant environmental conditions; and, second, the study of the activity which represents a response to changes in the environment. Studies of the lower animals by Stier (1930), Crozier (1934), Richter (1922, 1927), Wolf (1930), and many others have shown that spontaneous activity has a rhythmic character, and that its rate is specific to different pure stocks. A number of studies have also shown that the amount of activity manifested by an animal is constant within well-defined limits. Skinner (1933), in his study of the adult rat, showed that if activity was prohibited during part of the day, the remaining part of the day showed a greater density of activity per unit time.

In the mammals, changes in the distribution of the basic activity rhythms seem to result from adjustments to the environment consequent upon the operation of the autonomic nervous system. Boldyreff (1916), Wada (1922) and others have shown the relationship of bursts of activity to the hunger cycle, and analysis of the data on changes in motor activity resulting from disturbances of the autonomic nervous system shows that these are coincident with other changes in sympathetic and parasympathetic functioning. These changes are ordinarily the result of changes in the environment, and, as we know, the bulk of these disturbing situations come from other animals. Indeed, the ordinary procedure used in studying autonomic changes in the cat is to tie the animal to a board and then bring a dog into the room. The interaction which then results is sufficient to bring about the sympathetic reactions which Cannon (1936), Bard (1934) and others have described.

In interaction, therefore, we have two factors, the basic activity rhythms specific to the organism which make up what is called spontaneous activity and the changes in that activity consequent upon the responses brought about by the actions of other animals.

Interaction is, after all, only a conditioned form of the basic activity pattern. The infant animal, whether mouse or human, is unable to interact. He learns to do so only as a result of having to respond to the actions of others. Many observers have described how animals learn to interact. Carpenter (1934), for example, has recorded many incidents in which the mother conditions the infant to respond to her actions, and all of us who have children know well the long process of dangling rattles, calling the baby's name, etc., by which we get the child to respond automatically to our actions and to adjust his actions to ours.

Although activity is controlled by centers below the cortex, as is shown by observations of decorticate and decerebrate animals, interaction itself is a cortical pattern, since we know that under conditions inhibiting cortical control, the capacity to interact is markedly altered. These include states of strong emotion, the influence of alcohol or sodium amytol, in general paresis, and in many types of brain lesions. My colleagues, Lindemann and Finesinger (1938), have shown how drugs affecting the autonomic nervous system change the objective aspects of speech production.

The significance of this physiological evidence lies in the fact that it provides us with an understanding of the mechanisms which control the interactions of individuals. When we deal with human relations, therefore, we are not limited to statements about "emotion" or "sentiments" with which to describe adjustment or the lack of it. We are able to measure this adjustment and understand the mechanisms which control its changes.

SOME APPLICATIONS

The utility of measurements of human relations from the clinical point of view is clear, although we have by no means explored all its possibilities. We are able, at the very least, to make explicit many of the intuitive judgments of the clinicians, and to give quantitative definitions to cases of psychogenic disorders. In other areas, these methods promise to have great value.

In the field of selection of personnel, for example, the measurement of human relations seems to have even greater usefulness, since it can there be used to increase the effective working relations of large groups of people. Not only is it possible to differentiate the normal from the abnormal person in an employment interview and thus to keep the behaviourally unstable out of an organization; it is also possible to select individuals for different positions who will be able to adjust to the persons with whom they have to work. First, take the measurement of the capacity for leadership as defined by the ability to obtain responses from a group of other individuals and to maintain that leadership continuously. Not only is it necessary in choosing individuals for executive positions to get people who actually can lead, but also to select leaders who will fit the requirements of the interactional situation in the organization. A man who will make a good leader on a ship or in a construction gang will rarely be able to reduce his rate of initiations to be effective as head of a research laboratory. Second, the capacity for adjustment differs in different individuals. Some persons can get along well with a large number of individuals who differ markedly in their interaction rates. Other persons can adjust well only to people with high activity rates, or conversely. Third, individuals vary widely in the speed with which they interact. Two persons having the same activity rate can have markedly different speeds at which they interact, some being slow and deliberate, other fast and quick-acting. The requirements of different jobs differ markedly in the amount of speed required. Slow-acting persons in a fast-acting team will upset the group, who have to adjust their rates on a different level than that to which they have been accustomed. Fourth, the degree of initiative shown is of great importance in selecting personnel for different types of jobs. People without a high rate of initiations make poor salesmen, for example. Fifth, the activity curve itself will determine the degree to which an individual is able to have a high frequency of contacts with others. People with low activity rates make poor executives, unless the job requires little interaction with others.

In the long run, however, the study of the interactional properties of the individual which makes up what we ordinarily call his personality, is only important in so far as it contributes the understanding of the relations of individuals in institutions, of the way in which organizations actually operate. Organization problems are, after all, our basic concern, because in organizations, whether family, factory, church, club or government, most of our existence is passed. The degree to which organizations operate effectively has, therefore, an enormous importance for ourselves, and the solution of the difficulties which arise within organizations depends upon the degree to which we are able to define them objectively.

It was for this that these methods of measuring human relations were developed. The study of organization is essentially the study of the relations of personalities to one another within the framework provided by the routine activities of the organization in question. These activities, as I pointed out earlier, determine to a large degree who shall initiate action and who shall interact, and upon their capacities for adjustment to one another depends the working efficiency of the organization. This is the field which many anthropologists call the field of culture and personality, and it is here that the measurement of human relations promises to be of the greatest value.

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NEW MEMBERS

ELECTED APRIL 23, 1942

ACTIVE MEMBERS

Bakhmeteff, Boris A., C.E., D.E., Civil Engineering, Professor, Department of Civil Engineering, Columbia University, New York, N. Y.

Burstein, Elias, A.M., Physical Chemistry, Research Assistant, Solar Energy Project, Massachusetts Institute of Technology, Cambridge, Mass.

Clark, Allan C., Synoptic Meteorology, Division Meteorologist, Pan American Airways, Atlantic Division, New York, N. Y.

de Smitt, Vladimir P., Scientific Hydrographer, Instructor, Meteorology, Climatology, Physical Oceanography, Columbia University, New York, N. Y.

Heines, Neal J., Solar Research and Meteorology, Fellow, National Association for the Advancement of Science, Research Worker, Paterson, N. J.

Hess, Victor Francis, Ph.D., Physics, Meteorology, Geophysics, Professor, Fordham University, New York, N. Y.

Kimball, James H., Ph.D., D.Sc., Meteorology, Meteorologist-in-charge, U. S. Weather Bureau Office, New York, N. Y.

Mattiello, Joseph, Ph.D., Chemistry, Technical Director, The Hilo Varnish Company, Brooklyn, N. Y.

Moxom, Walter J., Meteorology, Regional Director, U. S. Weather Bureau, Region No. 1, New York, N. Y.

Quinn, Walter J., M.S., Electrical Engineering, Physics, Meteorology, New York, N. Y.

Roe, Anne, Ph.D., Psychology, Independent Consultant, New York, N. Y.

Sheridan, Laurence W., Ph.D., Meteorology, Chairman, Department of Mathematics, College of Mt. St. Vincent, New York, N. Y.

Trautz, Otto R., Ph.D., Chemistry, Assistant Professor, New York University, New York, N. Y.

ASSOCIATE MEMBERS

Calvin, Melvin, Ph.D., Chemistry, Assistant Professor, University of California, Berkeley, Calif.

Carpenter, Charles, D.E., Chemistry, Southland Paper Mills, Inc., Lufkin, Texas.

Carroll, Burt H., Ph.D., Chemistry, Research Laboratories, Eastman Kodak Company, Rochester, N. Y.

Cobb, R. M. Karapetoff, M.S., Chemistry, Chemist-in-charge of Research Laboratory Lowe Paper Company, Ridgefield, N. J.

Cole, L. E., Ph.D., Psychology, Professor, Oberlin College, Oberlin, Ohio.

Cooper, William Charles, Ph.D., Chemistry, Executive Chemical Engineer, Barrett Division, Allied Chemical and Dye Company, Jenkintown, Pa.

Copley, M. J., Ph.D., Physical Chemistry, Principal Chemist, Eastern Regional Research Laboratory, Chestnut Hill, Philadelphia, Pa.

Coster, Herbert Marvin, B.S., Chemistry, Superintendent of Acid Plants, U. S. Naval Powder Factory, Indian Head, Md.

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SECTION OF GEOLOGY*

MAY 4, 1942

DOCTOR GIRARD WHEELER, Chief of Subsurface Exploration Section, N. Y. C. Department of Public Works: *Recent Subsurface Explorations in New York City.* (This lecture was illustrated by lantern slides.)

The Department of Public Works was organized under the new City Charter in 1938. In 1939, the Subsurface Exploration Section was established in order to provide soil, rock and groundwater data for designs made by the Bureaus of Sewage Disposal, Architecture and Bridges.

Because so large a part of New York City is covered with buildings or pavements, it is usually necessary to obtain subsurface information by means of borings. In soil or soft rock, spoon ("dry") samples are obtained. In hard rock, cores are obtained by diamond drilling. Since the borings are made in order to obtain data which are not clearly known in advance, it is sometimes difficult to arrange an equable boring contract. For this reason

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*The Section of Psychology met May eighteenth. No abstract was received from Doctor David Wechsler, who gave an address entitled, "Psychological Aspects of Projective Techniques, with special reference to their Military Application."

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the policy of the Department of Public Works has been to do this work with its own equipment and forces, which continuously operate five drilling rigs. During the first 26 months of operation, 710 borings were made on 65 project sites. A total of 40,578 feet of soil and rock was drilled at a cost of $80,440, or $1.99 per foot. This cost of exploration represents 2/10ths of 1% of the cost of the engineering projects involved.

This extensive sampling of the subsurface of New York City has provided an unusual opportunity for collecting geological material and correlating it with current engineering needs and practice. As geologist, the writer has found many duties which it seems worth while to describe briefly. In general, the chief function of the geologist has been to classify, to interpret and to interpolate. For example, a well known source of trouble in construction is the occurrence of boulders. Engineers are not always able to appreciate the significance of several large boulders encountered by borings in a compact mixture of sand, gravel and clay. The geologist, on the other hand, may well interpret such an occurrence as till, and if this be the case, he may predict the probable existence of many boulders.

In a closely built-up city such as New York, the exact elevation of water is of considerable importance on almost any engineering project. Hence, another function of the geologist has been to interpret ground water records and predict on the basis of such records.

Since all engineering projects rest upon soil or rock, whose "presumptive bearing capacity" is defined in the New York City Building Code, it is incumbent upon the geologist to assist the designer in predicting the behavior of soil or rock under given methods of excavation, construction and loading. It quickly became apparent, as our subsurface work progressed, that precise quantitative estimates were necessary. This, in turn, required the installation of a soil mechanics laboratory. As a result, the designer is now provided with preliminary geological estimates of soil compressibility, shear strength, pre-consolidation, etc., and, as boring samples are obtained, he is given valuable laboratory test results.

This combination of qualitative geological analysis and quantitative laboratory analysis is a most satisfactory one. Once a given soil, for example the Gardiners clay, is shown by test to have been pre-consolidated by a glacial ice load as much as 3 or 4 tons per square foot greater than the present soil load, then a prompt geological recognition of “Gardiners clay” becomes of immediate value to designers.

Another obvious design need has been a correlation, in engineering terms, between soils as described in geological publications and the behavior of these soils under stress. To satisfy this need, an engineering soil-province map of New York City has been made with the following tentative provinces:–

Provinces	*Sub-provinces*
1. End Moraine Belt	
2. Outwash Plain	
3. Ice Recession Complex	3a. Lower Manhattan
4. Shore Zone	4a. Harlem River
	4b. Flushing Bay
	4c. Newton Creek
	4d. Gowanus Canal

Each province has been described in terms of soil engineering. By way of illustration, the description of the End Moraine Belt may be quoted:-

“Surface Soil:	Mixed material, including boulders, gravel, sand, silt, clay. Commonly unsorted “till” or boulder clay with wide dispersion of sizes. In places well bedded and sorted, sand or gravel.
“Bearing Capacity:	Building Code, 2–6 tons per sq. ft. Common design, 3–4 tons per sq. ft. Ultimate capacity, 4–10 tons per sq. ft.
“Subsoils:	Chiefly sand and gravel.
“Remarks:	This soil province contains some of the best bearing material in New York City. A

> good guide is often found in existing structures on adjacent property, but there are occasional irregularly distributed ponds and marsh deposits, usually of shallow depth. Groundwater table commonly deep, but owing to the presence of impervious layers or lenses there are occasional perched water tables near the surface. A few borings may be sufficient for exploration, carried to relatively shallow depths, chiefly to ascertain fill and groundwater conditions. Piles seldom required. Bedrock out of reach (except in parts of Staten Island)."

Still another function of the geologist has been to help in determining the proper relations between subsurface exploration data and the specifications of the subsequent foundation contract. This determination is of importance in order that the contractor may understand clearly just how much is known or not known regarding subsurface conditions and just how he is to be paid for footing excavations, piles or caissons, etc.

In consequence of the fact that it is difficult to formulate a perfect subsurface contract, and because legal controversies often arise, it is not infrequently the case that the geologist is called upon to give expert testimony in court. Such testimony is greatly strengthened, when the City is defendant, by offering in evidence actual boring samples obtained by city employees.

Finally, geological information has been utilized by the City in purchasing or selling land, for the value of land often depends in part upon the quality of soil or rock present.

The foregoing paragraphs have treated in general terms some methods of application of geology to engineering. Naturally, these explorations have shed some light on a number of purely geologic problems.

An interesting example is the origin of the extensive zone of deeply weathered gneiss in the Newtown Creek area. The bedrock in this area is thoroughly decomposed to depths of as much as 80 feet. The rather soft brown and white residual clay has been previously observed by Kerr (1), who subscribes to the Humphreys and Julien (2) explanation of leaching by corrosive action of preglacial peat bogs. It would seem reasonable to add the hypothesis that this clay represents the uneroded product of pre-Cretaceous weathering. This explanation is supported by the presence, in close proximity to the decomposed gneiss, of the lowest Cretaceous beds. Such an hypothesis would not necessarily vitiate the "bog weathering" concept, but would change very greatly the geological age to which the weathering is ascribed.

Another problem is to account for the breach in the terminal moraine at the Narrows between Brooklyn and Staten Island. As far as the writer is aware, there is no known evidence which would rule out the possibility of end-moraine deposits actually being present beneath the Narrows. On the other hand, borings taken in land and water off Owl's Head, Brooklyn, reveal a pebble lithology which suggests the former presence of a Hudson Valley ice tongue at this point. If this should prove to have been the case, it might well explain the breach in the moraine at the Narrows, since a late glacial valley tongue could easily remove former end-moraine debris.

Still another problem is to explain and correlate the sediments deposited in the New York Shore Zone areas after the last melting of glacial ice. At a number of locations beneath the soft surficial organic silts and clays of the waterways there have been found deposits of loose sand, 10–30 feet thick, which, in turn, are underlain by varve clays or varve-like clays. The clays, the loose sand and the soft silt all appear to be post-glacial in origin; but the genesis of the sedimentary types is not clear, nor is the precise dating. As yet, no fossils have been found in either clays or sand. Their absence might be accounted for by deposition in ponded areas shut off from the sea.

It is interesting that at the many sites thus far bored to rock, no trace has been recognized of two distinct till deposits. This apparent anomaly is cited, not so much to suggest that ice advanced over the New York City area only once during the Pleistocene, but to emphasize two other possible explanations: first, that a second ice advance may have obliterated earlier till deposits; second, that since till is upon occasions difficult to recognize in boring work, it may have been penetrated without recognition.

By far the most interesting question is the relationships of the varied soils which are commonly called "Gardiners Clay", or "Sankaty" of Veatch (3). For example, on the west side of Flushing Bay, at the site of the World's Fair, the succession of strata is, in descending order: 50–80 feet of very soft organic silt and clay, 10–30 feet of very compact yellowish sand and rounded quartz pebble gravel with broken shells, 20–60 feet of soft to moderately stiff varved clay, several feet of till with trap boulders, and bedrock. The varved clay occupies a position which would be appropriate for Gardiners clay, except that it is not overlain by a till. That the varved clay actually represents lacustrine or very quiet water deposits is hardly to be doubted. In its upper portion, approaching the shell-bearing gravel, it contains irregular zones of red fine sand, such as is characteristic of many Pleistocene sediments derived from the Newark red beds of New Jersey. One may visualize a pond filling and shoaling, but it is not so easy to explain the compact shell-bearing gravel at elevations of −60 to −80.

Crossing to the east side of Flushing Bay, the uppermost strata are 60–80 feet of soft organic silt, underlain by 5–10 feet of compact sand and variegated clay, and a thick deposit of very stiff thinly laminated carbonaceous clay containing carbonized layers and pyritized plant stems. One can hardly doubt that the sand and variegated clay are the equivalent of the shell-bearing gravel occurring on the west side of the bay, or that the carbonaceous pyrite-bearing clay is the equivalent of the varved clays on the west. The surmise comes readily to mind that the plant-bearing clay may represent bog deposition in an area bordering on lacustrine deposition of varved clays, and that there are wave-action products in the shell-bearing gravel and weathering products in the variegated clay, both of which occur at elevations −60 to −80. A comprehensive explanation of these deposits, however, must await further study and additional borings.

On the north side of Long Island Sound, a short distance from Flushing Bay, typical shell-bearing gray Gardiners clay is found at elevation −20 beneath bouldery till.

In the Newtown Creek area, at elevations appropriate to the Gardiners, sandy laminated clay with pyrite and carbonized wood occurs immediately beneath bouldery till.

In the Brooklyn Navy Yard, beneath till is shell-bearing Gardiners clay.

If all of these clays are Gardiners, then one is forced to the concept previously advanced by Veatch (4) that "These sediments therefore vary from truly swampy deposits, on the one hand, to relatively fine sands and clays, which show no trace of swamp origin, and which contain shallow water mollusks, on the other..." But the clays of Flushing Bay are somewhat unusual in that they are varved, and moreover do not seem to have been over-ridden by ice, hence must find special explanation. Perhaps there were small "driftless" areas near the Long Island terminal moraines, or perhaps the Pleistocene Gardiners is being confused with either post-Gardiners clay or residual pre-Pleistocene clays of somewhat similar composition. Just as in soils engineering one quickly adopts the principle "make one more boring than is necessary,—otherwise, there will be one boring too few," so in New York City geology one must always await with patience the results of "one more boring."

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SECTION OF BIOLOGY

May 11, 1942

Raymund L. Zwemer, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York City and the Instituto de Fisiologia, Universidad de Buenoi Aires, Buenos Aires, Argentina: *Uniformity of Responses to Potassium of Various Argentine Animal Species.* (This lecture was illustrated by lantern slides.)

The paper is a continuation of work presented before this section of the New York Academy of Sciences in December, 1936.

In attempting to determine what effect a prolonged increase in body fluid potassium might have on an individual, Dr. Truszkowski and I (1936,1938) were forced to abandon the techniques used up to 1935 and devise two new ones.

Aside from the study of ion changes in the fluids bathing isolated organs and tissues, the mode of potassium administration to the intact animal at that time was either by mouth or by vein. The absorption of the orally administered dose proved to be too slow to produce a measurable increase, while the intravenous injection of KCl solutions produced death at once, or a rapid return to normal levels. Continous intravenous methods involve many complicating factors difficult to control accurately. We, therefore, adopted intraperitoneal injection of a hypertonic solution of KCl which permitted absorption slow enough to be studied yet rapid enough to produce easily detectable changes in plasma K concentration.

It was also necessary to do micro determinations in large numbers in order to get curves and not just before and after samples. We thus obtained plasma K curves for various doses of K salts in solutions.

Potassium changes influencing cell physiology and pathology have been carefully studied during 1941 in Professor B. A. Houssay's laboratory with the kind collaboration of Drs. F. B. Arrighi, V. H. Cicardo and J. B. Odoriz.

A criticism that needed to be met was the possibility that clinical symptoms accompanying the K rise might be due to the anion (Cl), the hypertonicity of the solution administered or the amount of the solution given. It was found that the same total amount of K per kilogram body weight was lethal, whether given as the 2%, or any per cent up to 10% solution. The chlorides of Na or Ca when given in similar concentration had no such effect. Extremely large amounts of NaCl solution were given with beneficial rather than deleterious effect on the K symptoms.

In all the species studied, the lethal dose did not differ by more than a factor of 2. The individual variations, in response to a given non-lethal dose, were as great within a species as were variations from one species to another. This confirmed results previously obtained in the United States and Poland. This lethal dose is about 1cc. of 10% KCl per 100 grams body weight of animal. We can express this as 52 mg. K per 100 grams. This should be compared with a normal muscle content of about 400 mg. per 100 grams, given by Dr. H. Burr Steinbach, as present in many different animal species.

K differs from most other toxins which have wide species variations. Notable contrasts exist between cold-blooded and warm-blooded animals, and for mammals between the rat and the guinea pig.

The 50% lethal dose is a better one to test the protective effect of various types of therapy and was used by Truszkowski and Duszinska in K adrenal studies.

The clinical symptoms common to all species were: (1), the gradual appearance of skeletal muscular asthenia; (2), circulatory disturbances grossly noticeable as edema and slowing of the heart; (3), disturbance of autonomic system; and (4), effects on the central nervous system.

Naturally, the anatomy and physiology of each species brought about minor variations in the general picture. This is seen in the description which follows for a single lethal dose (Multiple doses will not be considered as they involve too many complications).

Mammals showed an initial marked stimulation of the autonomic nervous system which might be due to local reflex action of the K on visceral sensory neurons.

Partial paralysis of hind legs within 15 minutes of injection is followed by progressive weakness of the abdominal musculature and of the fore-limbs until, eventually, the animal is unable to stand or right itself, if turned over. The edema is manifested by puffiness of the face especially around the eyes. This is particularly noticeable in the guinea pig, which, also, shows the respiratory difficulties typical of anaphylaxis. The anesthetic effect on the central nervous system can be detected grossly by the disappearance of reflex responses to peripheral touch. This will be dealt with more fully later.

In birds, the ability to fly disappeared before the ability to stand. This reverses the limb order of mammals and is interesting in connection with functional evolution.

Of the reptiles, snakes were used because large quantities were easily available.

Ophis merremi (hooded snake). The name comes from the habit of flattening the neck by elongated ribs—after K, the whole body was flat but would round up on stimulation.

Crotalus terrificus (Cascabel). rattlesnake. To watch one strike in slow motion after K is an experience.

Bothrops alternatus (Pit Viper) Urutu. May reach five feet, but as with most snakes, weigh surprisingly little for their length.

Bothrops neuwiedii (Maximilian's Viper). This variety showed marked convulsions.

Factors influencing the constancy of toad, *Bufo arenarum* (Hensel), response to K were hormonal. Removal of either adrenals or pituitaries lowered resistance (Houssay). In normal toads (of which we used over 500), the sequence of events after K were: (a) slightly flaccid abdominal muscles (15 mins.); (b) a slow jump reflex; (c) the animal cannot stand up well, but head sinks down; (d) when put on its back, it turns over with difficulty; and (e) cannot turn over but there is a contralateral reflex when one limb is touched.

With sub-lethal doses the recovery from these is rapid.

Later, there is no response to any stimulus and the animal appears dead with the entire body absolutely limp. However, there may be an extremely slow heart beat and there may be a recovery from this state after as much as 5 hours. During this absence of respiration, the oxygenation must occur in the skin capillaries.

Although temperature did not greatly influence the ultimate outcome of K injections, it did alter the symptoms.

At 37° Centigrade (our body temperature), the toads were much more active than at a normal room temperature of 20° to 22°. Their ability to jump higher and more often was especially marked if the animals were put in a warm room for an hour before injection of KCl solution. As the K effect occurs, their actions were slower and muscle tone less. If the toads did jump, they rested longer before the next jump. Recovery, after sub-lethal K, was more rapid in the warm room.

In the cold, 0° to −3° C., the toads are very sluggish in all their movements and the righting action of the animals after being placed on their backs must be watched for a longer period of time. There is a difference in the "feel" (i.e., the muscular tension of abdominal muscles) of animals slow from the cold and slow from potassium. The flabbiness from K predominated over the stiffness from cold as the syndrome progresses.

In the cold, it was also difficult to determine death. Animals, put aside as dead because respiration had ceased and the heart beat could not be palpated, recovered later even though they had apparently been dead.

These clinical observations on lethal and sub-lethal K actions led to some special studies on the toad.

The first, with Dr. Cicardo was an attempt to determine whether the asthemia was due to a failure of nerve conduction or of the myo-neural junction. The condensor method of Lapique, using silver-chloride electrodes, was used to determine the rheobase and chromaxia of nerve and muscle. Central nervous excitability was measured by the voltage necessary to produce a contralateral reflex, using a condensor discharge with a frequency of 20 stimuli per second. We found a definite diminution of the reflex excitability which coincided with observable asthemia of the animal. The voltage necessary to produce a contralateral reflex increased until no reflex was obtained. Recovery of all animals was observed after 1 ml. of 6% KCl per 100 g. Neuromuscular excitability was not related to the asthemia except in the 17% in which a reversible curarization appeared. Muscle showed an unusual K effect which we are unable to explain. The rheobase, or current potential necessary to produce a contraction, was high and continued to rise 8 to 24 hours after the recovery phase. This lack of response to direct stimulation was particularly notable in that the muscle responded to normal stimulation of its nerve and there was a complete integration of spontaneous movements in the animal. This phenomenon was not due to polarization of the electrodes since it was also determined with alternating current.

These results led to a study of spontaneous central nervous activity with the cathode-ray oscillograph, recording what are commonly called brain waves. With Dr. Odoriz' apparatus and skill, continuous photographic records were made which showed that normal brain activity was at first stimulated, then inhibited, and finally stopped, as the K content of the blood increased after intraperitoneal injection of KCl solution. By placing electrodes in various locations, it was found that inhibition of spontaneous activity by K occurred first in the brain and 10 to 15 minutes later in the cord. Recovery was in the reverse order. Spinal reflexes could be obtained at a time when spontaneous brain activity was stopped. To put it simply, there were times when a toad could not move of his own accord, but would attempt to jump, if poked. It might then become completely non-reactive but an hour later be hopping around at a great rate.

The difficulties encountered in determining when a heart had really stopped beating led to the use of electro-cardiographic recordings of the heart beat before and after K. Dr. Arrighi had developed a technique of holding the electrodes next to the right fore-and left hind-limb of the naturally moistened toad, in such a manner that the animal was temporarily quiet enough to permit electrical recording of the heart beat. During the period that the persuasive powers of K were acting on the CNS, no restraint was necessary and beautiful records were possible. These showed a slowing of the rate not only of the heart but of the sequence of each part of the heart. A perceptible beat thus became a slow motion event. In animals dying from a lethal K dose, the thorax was opened and the contractions of sinus, atrium, ventricle and aortic bulb could be seen separately. The natural recovery from this state after sub-lethal injections could be speeded up by treatment with NaCl.

These special studies when correlated with the general symptoms observed in all species injected, lead us to reaffirm that the uniformity of response to K is a fundamental biological phenomenon of living cells. The exact location of a maximum effect is dependent on the amount of K given and the region and speed of administration.

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SECTION OF ANTHROPOLOGY

MAY 25, 1942

DOCTOR C. R. CARPENTER, The Pennsylvania State College: *Characteristics of Social Behavior in Non-Human Primates.* (This lecture was illustrated by motion pictures.)

It is the purpose of this lecture to describe some of the important characteristics of non-human primate behavior. I mean by *behavior*, as used in this lecture, observable patterns of movement, in one or more individuals, as the activities occur in complex stimulus fields. The consequences of these actions are also implied. Social behavior refers to the reciprocal interactions of two or more animals and the resulting modifications of an individual's action systems. The term, *characteristic*, will be used somewhat synonymously with the word, *mechanism*, and means a prominent action pattern or action principle which persists in individuals of a species and has adjustive value.

The following assumptions are believed to be basic to later discussion:

(1) The study of the phylogenesis of behavior and social relations on the non-human primate level will facilitate the understanding and control of human activities. This approach is likely to reveal archaic behavior mechanisms which are organically functional on the human level.

(2) The kinship of non-human primates to man makes it reasonable to assume that the study of these types may yield data which can be readily employed in understanding basic kinds of human motivation.

(3) It is obligatory, if some problems of behavior and social relations are to be studied validly, not only to study *animals as wholes* but to study *whole animals in their natural groupings in that environment which has been operating selectively on the species and in that environment to which the species is fittingly adapted.*

(4) All behavior, including that called cultural, has ana-

tomical and physiological counterparts. The cause and effect relationship is reciprocal and not uni-directional. Through learning, for example, modifications may result in the organism, particularly in sub-microscopic structure of the brain, which persistently modify the animal's subsequent behavior and social reactions.

The Characteristics of Interactance.

The principle of interactance holds that primates of a group interact each with the other. Theoretically, each individual in a group consisting of 18 howler monkeys has the possibilities of interacting with every other individual. By applying the formula $N\frac{(N-1)}{2}$ it is found that there possibly would be 156 paired interactions. However, the actual intra-group interactions are not as uniform and simple. Animal A may react with animals B, C, and D but also animal A may interact with animal E who in turn reacts to animals B, C and G. When one begins a detailed analysis of a naturalistic group of monkeys or apes it is found that the group is *organized*, that it has structure and pattern and that limits (boundaries) are set to possibilities of movements and relations of each individual. The individual in the group has a status dependent on age, sex, behavior characteristics, e.g., strength, dominant motivation, previous conditioning and disposition towards other members of the group. The individual has prestige depending on previous status. Though members of naturalistic groups of monkeys and apes do interact by reciprocal stimulation and response, they do not have freedom of movement. Individuals are only autonomous within the structure of the social group but their ranges of behavior are strictly determined.

The description of natural groups of monkeys and apes requires classification of qualitatively different kinds of interactions. There are at least eleven of these for the average primate monkey group:

(1) Male-Male
(2) Female-Female
(3) Male-Female

(4) Male-young (of different sex and age)
(5) Female-young (of varying ages and sex)
(6) Young-young
(7) Relations of non-group living animals to organized groups.
(8) Group-group relations
(9) Relations of an individual to the total group configuration
(10) Inter-species and inter-genera relations, and
(11) Relations of animals to their ecological matrix.

Characteristics of individuals, other than those of age, sex and social status, enter to further complicate the description. When an observer becomes acquainted with a group like No. 1 on Santiago Island, he learns that *no* individual is exactly alike another. There are observable variations in amounts and kinds of general activity, variations in responses to other members of the same group. These individual differences are reflected or expressed and amplified on the level of complex social behavior.

If the observational focus be narrowed and a single *social bond* (relation) between two monkeys be considered, that bond is found to be a complex expression of interactions between the two individuals. The quality of the social interaction will vary depending upon the prepotency of motivation. The two general possibilities of interactance are positive and negative or affinitive and antagonistic. Infants at play, at one time, shift quickly to fighting, at another time. The male which attacked a female a few minutes ago may now permit her to groom him. The female which permitted the infant to nurse now repells its approaches. The results of the interacting of various kinds and degrees of *social drives* and *social incentives* expresses for the time being the general interactions of the two animals. The general reactivity of two (or more) monkeys in a natural situation is usually rather accurately expressed by spatial arrangements; i.e., distance separating individuals or group scatter.

The social interactions are modifiable through learning and/

or conditioning. Strong positive relations may develop between two or more individuals and these animals react in a consistent and characteristic fashion to each other. A very young male animal that is temporarily non-group living, may avoid approaching one group but react more positively to another. It may, through following the latter persistently and at an increasingly closer range, finally be accepted into the group.

An important feature of this non-human learning of social reactions is that of the recognition of individual differences and their appropriate reaction. This is possibly a feature of the social behavior of most organisms, but it is an especially prominent feature of the social behavior of all primates which I have observed. Each monkey in Group I (Santiago *Rhesus* Colony) reacted differently to 160 ("Diablo") than to any of the other group males (4). Only a short time may pass after a "foreign" female has entered the sphere of a well organized group until she would be recognized and usually attacked and driven from the group or killed.

Thus, it is clear that an analysis of the characteristics interactions within a primate group becomes extremely complex and significant in the study of social behavior.

The Characteristic of Grouping Patterns

When an *adequate sampling* of natural groups of any non-human primate species is studied, it is found the groups show a central tendency as well as a range of variability. The average group size for howlers is 17.3 ± 6.8. The average group or family of gibbons has 4 individuals and ranges from 2 to 6 (1). The same group size is characteristic of the siamang. The age of the grouping may relate to its size for, after groups have been long formed and approach the upper limits of size, group splitting or *apoblastosis* occurs, the parent group is reduced in size, and the nucleus of a new group (or groups) is formed (3). (The process is very analogous to cell mitosis).

Studies of grouping patterns show also that usually there are more intra-group males than females. For the howler, this *socionomic sex ratio* is about 1 : 3. While, for the spider monkeys, the ratio is approximately 1 : 2, (2) and it is estimated that a 1 : 3 ratio holds approximately for *Rhesus* monkeys. In gibbons which have a characteristic family grouping pattern, the ratio of adult males to females is usually 1 : 1 although there are few exceptions.

When the organization of a sample population is studied, including animals which do not, at the time, live in organized heterosexual groups, variations characteristic of species also are found. Howler males live temporarily alone. Excess spider monkey males live in unisexual male groupings, as do the adolescent and non-group living adult *males* of *Macca mulatta.* It is believed that isolates, both male and female, occur in the gibbon (*H. lar*).

If a unitary or approximately unitary secondary sex (at birth) ratio is assumed for those species which show fewer breeding males than breeding females within groups, then processes of social-sexual selection must be operative. Either (1) some individuals are never allowed to reproduce themselves, or (2) males only have access to females for limited periods of their life span. Available data do not permit a clear cut answer to this problem. I have found that, when male groupings of spider monkeys are collected, young adult, prime and old ages may be represented in the same group. The adolescent males, perhaps, from about 4 or 5 to 6 or 7 years of age, live in unisexual male groups, while old senile individuals may usually live alone or sometimes with one or two very young males. Males in their prime, usually but not always, live in the organized heterosexual group. Individual males may live in groups, however, *and yet not have the chance to reproduce,* for, in addition to living in the group, he must have a requisite degree of dominance status or prestige before he can reproduce.

The Characteristic of Dominance

There is no question but that groups of monkeys and apes are organized around several dominance gradients. By dominance gradient, I mean the order of arrangement of several individuals along a line from low to high dominance. The dominance of one species may vary markedly from that of other species. Furthermore, the dominance status of an individual in a group, as gained by aggression and maintained by aggression, may blend with a prestige factor, i.e., a kind of perseveration of a previous dominance status.

The variability of dominance or aggressiveness from species to species is shown when baboons are compared with howlers, or when langurs are compared with spider monkeys. There would seem to be a genetic basis for this complex kind of behavior which is manifest by the relative degree to which females, food, preferred positions, etc., are exclusively possessed by an individual. In baboon harems a number of females are exclusively possessed by the overlord. Langur males, too, are very autocratic and intolerant of other males. By contrast, howlers tolerate males of the same group. *Rhesus* males of the same group show a sharp dominance gradient but they, like howlers, do not possess receptive females exclusively. Rather, in both kinds, a type of *rotational mateship* occurs during the period of a female's estrus (1 and 4).

In almost all natural groupings which have been carefully studied, the male or males play the most prominent role in controlling the group. There is clearly a masculine dominance, with the possible exception of gibbons, in which is found equivalence of sexual dominance.

This does not preclude the fact that, among the females, as among males, there is also a dominance gradient. As compared with that of the males, this female dominance gradient is of a lower slope; i.e., females have less absolute dominance than males and they seem to differ to a smaller degree. Nevertheless, the female gradient overlaps that of the males. This means that some of the most dominant females are more dominant than some of the least dominant males.

When dominance is studied in immature animals it is found that there, too, an *unstable* gradient exists. Indeed, the status of an individual *Rhesus* monkey begins to be defined during its infancy, after it has begun to engage in social play.

These dominance gradients importantly affect social controls in the groups. The group scatter, or spacing, during feeding, the order of locomotion and the order of access to estrous females are a few of the behavior modalities in which the order of response is dependent upon dominance and prestige statuses.

That the male dominance gradient is one of the main axes of an organized group, was shown recently by *an experimental field study* (unpublished). The study demanded continuous observation of Group No. 1 of Santiago Colony for 5 days during each week for a month. Systematic records were kept of all behavior classified as having any degree of aggressiveness. The first week was a control period after which the supremely autocratic male (Diablo) was trapped and held captive. During the third week, No. 174, or male No. 2 in the dominance hierarchy, was trapped and held captive and No. 150 or the No. 3 in dominance order was next trapped. Accumulative systematic records showed changes following each removal.

As soon as the Number One dominant male was removed from the group, the *territorial range* of the group was markedly restricted. The group organization became more fluid and there was an increase in *intra-group conflicts* and fights. Finally when Nos. 1, 2 and 3 were removed, males Nos. 4 and 5 shifted dominance positions. No. 159, previously dominant over 173, was now superseded by 173.

When the three captive dominant males were released, a dozen females surrounded "Diablo" (No. 1 dominant), while fewer clustered around Nos. 174 and 150. There was marked excitement of a peaceful sort. At one time, six females were photographed grooming Diablo, while three and two respectively groomed 174 and 150. After a marked disruption lasting three weeks, the group was suddenly restructured when the dominant males were released.

The Characteristic of Territorialism

All types of primates, which have been adequately studied in the field, have been found to show the phenomenon of territorialism. Organized groups have restricted and sometimes quite limited ranges. In some instances these ranges are exclusively possessed by a single group, while, at other times and places varying degrees of overlappings are found among neighboring groups. It would seem reasonable to conclude that conditioning and learning importantly determine this territorialism and that these processes relate to the spatial locations of incentives which satisfy basic needs of the individual and the group.

A group of gibbons in its territory is less *tolerant* (more resistant) of another group of gibbons than it is of a group of langurs or macaques. *H. agalis* and the siamang have been observed to feed peacefully in the same tree. This would not happen without conflict with two groups of the same species. The conclusion may be drawn, therefore, that competition is most keen between organized groups of the same species. And this is so, even though the different species have similar food preferences.

The relative dominance of groups and territorial ranges interact. The question has often occurred to me as to why one group, for a while, was more dominant or had right of way over another group and hence had wider freedom of movement. The Santiago studies mentioned above have thrown some light on this question. Group dominance does not relate to the size of the group, for Group 1 was only about one-half the size of Group 2 and contained fewer males (7 to 10), but Group 1 always could take precedence over Group 2. The results of field experiments suggest that the determining factor of inter-group dominance is the *relative dominance of the autocratic male in one group in relation to the autocratic male of another group.* In the experiment, Group 1 lost its freedom of movement and range as soon as "Diablo" was caged.

Characteristics of Interdependence

Charles Darwin and many natural historians following him have so emphasized *"the struggle for existence"* and competition that the facts of *interdependence* in animal societies have not been accurately and fully represented. As a result, the formulated theories and principles of social behavior are faulty, since they are built on inadequate factual bases. An illustration of this is the common expressions which hold that those aspects of human behavior which are untenable, according to a prevailing ethical system, have been derived from an animal level. Extreme egoism, sadism and masochism are frequently thus designated. By contrast, those activities which are ethically accepted, such as altruism, strong emotional affection and cooperation, are attributed to man's higher intellectual processes if not to super-human origins. The naturalistic approach to the study of social behavior leads to the conclusion that *anlagen* or genetic origins of *all kinds* of human behavior, competitive and cooperative, egoistic and altruistic, affinitive and antagonistic, have roots in the pre-human levels of the phylogenetic series.

The survival and reproduction of groups of monkeys of a species depend on the social coordination—one may say cooperation—of all the individuals of the group. The survival is quite as dependent upon the *epiorganism* as on the organism.

Bi-sexual reproduction requires a degree of coordination of two or more individuals. Particularly, on the non-human primate level the young are born without the capacity for independent survival. They are cared for and protected by their mothers in *Rhesus* monkey groups. Otherwise they are killed usually by members of their own parent groups. The play of young monkeys is highly cooperative and it is during that play that many natural reaction tendencies are modified, matured and established as individual habit systems. Defensive actions may involve the close coordination of all group members in a concerted pattern of attack. In these attacks, individuals are killed, but this is incidental to the fact that the group survives and the species is perpetuated.

The Characteristics of Integration Coordination

A given number of monkeys and apes does not make or equal what I have been calling a group. Suppose we try this experiment: Raise in isolation animals of the species, but of the right sex and age to compose a group which meets the requirements of the formula for the average group characteristic of a species. These individuals will then be released together. What will happen? Some may so fear others that they flee. Some will be antagonistic and fight. Others will form into groups and remain together. In general, the synthetic group experiment will not fall together, as had been hoped or predicted, into a single organization. Why? Even though the social drives are operative and social incentives are present, the monkeys have not been conditioned to each other. They have not been socialized—i.e., they have not learned to make fitting responses to each other as complexes of stimuli. What is lacking is what I have called *integration.*

Social integration is conceived to begin with birth and to involve definable processes of social learning and adjustment. These processes are organic and involve the expressions and satisfactions of physiological drives. From one viewpoint, effective social integration of an individual conditions it in a manner to make it responsive to the communicative acts, motor expressions including gestures, and vocalizations. These communicative acts, involving specific stimuli patterns and fitting responses, constitute the core of group coordination. Let it be remembered that the stimulus aspects of communicative acts cannot be operative except on a background of social integration—i.e., animals which are conditioned to each other.

In order to emphasize the fact of the dependence of even complex social behavior in physiological processes, I shall, in conclusion, describe an instance of nonconformist behavior in a female *Rhesus* monkey (4). In the Santiago Colony, female No. 144 was a member of Group 5 which consisted of 13 animals. The Group had only one male which was very non-aggressive and lacked a strong sexual drive. When the female No. 144 became sexually receptive and had her drive frustrated in her native group, she left Group 5 and joined Group 1 (Group 5 had the lowest, Group 1 the highest inter-group dominance). At first, she moved tangentially to the group and was repeatedly attacked by the group members, males and females. Later, she formed a consort relation with No. 159, the male 4th in the dominance hierarchy, and then with 150, 3rd in the dominance order, and subsequently she was accepted by 174 and 160 or those males of highest dominance status. Upon termination of her estrus, she was driven out of Group 1 and subsequently returned to Group 5, her native group. It should be noted that the motives which compelled 144 to leave her group and the conditions upon which she was temporarily admitted to the semi-closed Group 1 were dependent upon the physiological conditions which underlie estrus. Thus, this complex non-conformist social behavior can be said to relate closely to the sex hormones, estrone and progesterone, as these affect the whole organism.

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